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of Unified Science

VOLUME I PART 2

EDITED BY

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Recent years have witnessed a striking growth of interest in scientific enterprise and especially in the unity of science. A new concern throughout the thinking world for the logic, history, and sociology of science led to this *Encyclopedia,* begun in 1938 under the editorship of the late Otto Neurath. The first volume, containing the first ten of twenty introductory monographs, is here presented in two permanently bound parts.

Part 2

PRINCIPLES OF THE THEORY OF PROBABILITY

ERNEST NAGEL begins the second part of Volume I with a review of the historical development and applications of the theory of probability. This is followed by a section on the calculus of probability and its possible interpretations, and a third section on the unsettled problems, specifically the logical problems posed by the frequency interpretation, and the relation of the concept of probability to the concept of degree of confirmation.

FOUNDATIONS OF PHYSICS

PHILIPP FRANK explores the logical structure of physical theories; classical Newtonian mechanics; heat, irreversibility, and statistics; the theory of relativity; light; mechanics of small masses (wave mechanics); and the structure of matter. Throughout the monograph the author considers various philosophical misinterpretations of both classical and contemporary physics.

COSMOLOGY

E. FINLAY-FREUNDLICH organizes this discussion of the cosmos in terms of

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I. The Materials for the Study of Probability

1. Introduction

The daily affairs of men are carried on within a framework of steady habits and confident beliefs, on the one hand, and of unpredictable strokes of fortune and precarious judgments, on the other. Our lives are not filled with constant surprises, and not all our beliefs are betrayed by the course of events; nevertheless, when we examine the grounds even of our most considered actions and beliefs, we do not usually find conclusive evidence for their correctness. We undertake commercial or scientific projects, although we do not know whether illness or death will prevent us from completing them; we plan tomorrow's holiday, although we are uncertain what weather tomorrow will bring; we estimate our budget for next year, although we are not sure whether the consequences of floods, droughts, or wars will not seriously throw it out of balance. In spite of such uncertainties, we manage to order our lives with some measure of satisfaction; and we learn, though not always easily, that, even when the grounds for our beliefs are not conclusive, some beliefs can be better grounded than others. Our claims to knowledge may not be established beyond every possibility of error, but our general experience is warrant for the fact that even inconclusive arguments may differ in their adequacy.

These observations are commonplaces. But they immediately lose their triviality if, by setting them in the context of a penetrating comment of Charles Peirce, we extend them to the procedures and conclusions of the various special sciences. The American logician once remarked that in the exact sciences of measurement, such as astronomy, no self-respecting scientist

will now state his conclusions without their coefficient of probable error. He added that, if this practice is not followed in other disciplines, it is because the probable errors in them are too great to be calculated. The ability of a science to indicate the probable errors of its measurements was thus taken by Peirce as a sign of maturity and not of defect. By his remark Peirce therefore wished to indicate that for the propositions in the most developed empirical sciences, no less than for those in the affairs of everyday life, no finality is obtainable, however well they may be supported by the actual evidence at hand.

The temper of mind which is illustrated by such an appraisal is itself the product of modern science and of a preoccupation with its procedures. It is based on the conviction that the methods of the natural sciences are the most reliable instruments men have thus far devised for ascertaining matters of fact, but that withal the conclusions reached by them are only probable because they rest upon evidence which is formally incomplete. The import of such an insistence upon the fallible character of science can be best appreciated by contrasting it with the classic conception of science, formulated in Greek antiquity and perpetuated in a powerful intellectual tradition. This conception of scientific knowledge was modeled upon the ideal of a completely demonstrative and absolutely indubitable natural science, such as Euclidean geometry was believed to be. It was assumed that the subject matter of genuine science was a realm of precise, unalterable laws, and that scientific knowledge, as distinct from belief, opinion, or mere experience, was be be equated with demonstrated knowledge. For such knowledge facts are not contingent, since they must be apprehended through their "reasons" or "causes," and the propositions which express them must therefore be "necessary." Furthermore, it was maintained that the "basic propositions" required as premises for demonstrated knowledge could be grasped by the intellect directly and infallibly and could be seen to be true with even greater assurance than any of the conclusions derived from them. The scientific enterprise was accordingly construed as the progressive apprehension of an eternal

order of necessary connections, so that complete certainty was the earmark of genuine knowledge. The changing and the variable could not be subject matter for science; they could at best be the concern of belief and opinion. Variability in the materials studied or in the outcome of measurements was taken to indicate either the obdurateness of subject matter to rational connections or the failure of thought to reach its proper objectives. In a word, experience in the sense of observation and experiment, since it could not yield necessary propositions, could not be the ground for scientific knowledge.

This ideal of science dominated the minds of the great pioneers of modern science and of many of their most illustrious successors; and it is this conception which forms the tacit premiss of many philosophic commentators upon modern science, such as Descartes, Locke, Leibniz, and Kant. It is scarcely possible to exaggerate the significant role which this ideal has played in intellectual history. In proclaiming the ideal of science to be *systematic* knowledge, the rationalist tradition has stimulated research and has led to the development of science as something other than an indigestible miscellany of dubious facts. On the other hand, the great services of classic rationalism cannot hide the fact that its theory of self-evidence rests upon an inadequate analysis of the methods of inquiry, so that it has frequently blocked the progress of inquiry and, though pledged to the ideal of clarity, has not seldom successfully courted obscurantism. Rationalism made complete certitude the theoretical condition for genuine science, but its belief that the latter was obtainable could be maintained only by neglecting or misinterpreting the approximate and contingent character of statements dealing with matters of fact. The long history of science and philosophy is in large measure the history of the progressive emancipation of men's minds from the theory of self-evident truths and from the postulate of complete certainty as the mark of scientific knowledge. Some of the major turning-points in that history consist in radically diminishing the class of statements certifiable simply by a rational insight into their truth. And some of its most dramatic moments have

occurred when the approximate and incompletely grounded character of allegedly indubitable propositions was recognized.

The forthright admission of the probable or contingent character of even our most soundly based beliefs and the emphasis upon the general reliability of the *methods* of scientific inquiry rather than upon its conclusions are characteristic of contemporary empiricism. For the traditional empiricism of Locke and Mill, which in intent was a revolt against the exaggerated claims of rationalism, accepted in all essentials the standards and preconceptions of the views it nominally opposed. But that admission of the probable character of our beliefs is not the outcome of a capricious decision: it is not a pronouncement made for the sake of wilfully opposing a historically powerful tradition, nor is it a thesis advanced for the sake of a special set of values and an ulterior conception of nature. That admission and that emphasis have been wrung from students as a consequence of their reflection upon the history of science and of a painstaking examination of its methods. Contemporary empiricists who maintain that our knowledge of matters of fact is "probable" do not thereby maintain that such knowledge is inferior to knowledge of some other kind obtainable by methods different from those the natural sciences employ. On the contrary, they maintain that "probable knowledge" is the only kind of knowledge we can find or exhibit, and that the methods and techniques of the sciences are efficacious and dependable precisely because they make available knowledge of that character.

2. Development and Applications of the Theory of Probability

Although the term 'probable' has been employed several times in the preceding section, no precise sense has been attached to it. It is one of the objects of this essay to assign a clear meaning to sentences which contain the term and its derivatives; but it must be admitted at the outset that an analysis of what is meant by 'probable', which would meet the unanimous approval of competent students of the subject cannot be given at the present stage of research. In the present and preceding

sections the statement 'Knowledge of matters of fact is probable' is to be understood in the rather loose sense that conclusions of factual inquiry are not in principle incorrigible, because the formal conditions for assuring the logical validity of those conclusions are not completely realized, and because statements having factual content are not logically necessary.

The doctrine that knowledge of matters of fact is only probable is one of the central theses of contemporary analysis of scientific method. The implementation of this doctrine with modern logical and mathematical techniques is relatively recent. But even during the heyday of classic rationalism the status of beliefs which fell short of its ideal of scientific knowledge was frequently and vigorously discussed. Out of the permanent needs which generated such discussions have grown the modern calculi of probability and the diverse interpretations and applications which the term 'probable' has received. The possible equivocality and unquestionable vagueness of the term are therefore in part due to the history of empirical science. The brief survey, to which we now turn, of some of the contexts in which the term is and has been applied aims to achieve three things: to emphasize the intimate connection between the development of empirical science and the growing need for a theory of probability; to indicate the great range of applications of the term 'probable' and so to provide the materials for a discussion of its meaning; and to serve as a convenient introduction to the issues and techniques under contemporary discussion.

a) Aristotle's logical writings formulate the rationalist ideal of science, but his biological works exhibit less exacting standards of scientific adequacy. His evaluation of the then extant theories of sexual reproduction is characteristically judicious; evidence, some of it observational, is presented in opposition to the Hippocratean doctrines and in support of his own views, but there is not even a pretense that the question is settled beyond further debate. His examination of the facts of heredity show him to be familiar with at least the crude elements of a statistical explanation of the similarities and differences between ancestors and descendants. The mechanism which he

suggested as an explanation for the observed facts was in essentials that of a shuffling and recombination of characters, so that only certain traits would normally recur. Even before Aristotle the principle of natural selection was advanced by Empedocles, though Aristotle had little use for it. No ancient mathematician developed a technique for handling statistical aggregates, and it is possible that the prevalent view of chance as an agent was an insurmountable impediment to a consistent working-out of a statistical view of nature which the theory of natural selection suggests. Nevertheless, passages in Aristotle and in the writings of the Ionians, Democritus, and Hippocrates which could be cited indicate that such a view was not foreign to the ancient mind.

b) Occasions for dealing with evidence which is not conclusive, but which nevertheless carries some weight, presented themselves in the legal and social transactions of both Athens and Rome. For example, there was a rule in Athenian courts excluding hearsay evidence, on the ground of the general untrustworthiness of reported statements as compared with the evidence of eyewitnesses. The courts of Rome took pride in deciding cases before them upon the basis of reason and the evidence of fact rather than caprice, and complicated safeguards were instituted to assure the adequacy of the evidence presented. Curious and distorted survivals of these appear in the formalistic rules of evidence of the Middle Ages. For example, two witnesses were required for a "full proof," the testimony of a single reliable witness counted as "half-proof," while a doubtful witness counted for "less than half." The object apparently aimed at was to convert the process of rendering a decision into a calculation of the "resultant force" of the testimony submitted. There was thus some basis in fact for Rabelais' portrait of Judge Bridlegoose, who made his decisions which were "correct" in the long run by throwing appropriately loaded dice. Years after, Leibniz was again intrigued by the possibility of a calculus of evidence, and the ideal of a quantitative science of proof has frequently hovered before students of probability. When the calculus of probability was finally developed, many

of its great masters, like Laplace, Poisson, and their followers, attempted to turn it to such a use, though with singularly poor success.

c) Although no individual knows the exact date of his death, he can reasonably expect a definite span of life. His expectations are based on statistical regularities manifesting themselves in large groups of men. The use of such statistical uniformities for predicting individual behavior illustrates a common type of "uncertain inference," which in recent times has become an exacting and important discipline; and it was exemplified in ancient practices as well. Various forms of commercial insurance existed in Babylonia, Greece, and Rome, and the Romans were no strangers to life insurance. Just how and on the basis of what kind of statistical information the various rates were estimated is now unknown, although it is fairly clear that the estimates were not arbitrary. For example, the rates on bottomry and marine insurance depended on the destination of the vessel and the season during which it sailed; and, although the careful gathering of vital statistics is a modern phenomenon, a census of populations was frequently made in antiquity for military and taxation purposes. While therefore the practice of insurance was not placed upon a sound basis until the end of the eighteenth century, it was built on a large body of factual information; and, even though the beliefs which rested on this information fell short of the classic ideal of science, they made possible the planning and execution of important policies.

During the Middle Ages the Italian cities saw the beginning of commercial insurance as a profit-making enterprise; by 1700 the business of insurance was rapidly developing in western Europe, with life insurance in regular demand a century later. These enterprises required to be supported by adequate statistical techniques, and in fairly rapid succession there appeared a number of important statistical studies. For example, in 1662 John Graunt showed how to employ the register of deaths, which began to be kept in London during the Black Death, to make forecasts on population trends; during the same century John De Witt, grand pensioner of Holland, and Halley, the Eng-

lish astronomer, concerned themselves with annuity problems. Halley laid the basis for a correct theory of the subject, and he showed how to calculate from the mortality tables which he constructed the value of an annuity on the life of a person of given age. While scientific knowledge in accordance with the rationalist ideal was not obtainable for these domains, probable knowledge was, and it became the guide to life.

d) The entire subject of statistical inference now called for a theoretical foundation. The need was supplied from an unexpected quarter—the theory of games of chance. Dice games played with ankle bones were popular in antiquity, and the ancients distinguished between the "likelihoods" of certain combinations of throws. They did not, however, develop any technique for assigning numerical measures to the different "degrees of likelihood." The quantitative study of games of chance begins with the modern period and was cultivated by a brilliant succession of mathematicians.

Solutions of special problems in the division of stakes and the placing of wagers were first given by Cardan and Galileo (sixteenth century); but the general attack on the theory which was involved in their analyses began with Pascal and Fermat (seventeenth century), who showed that all the special problems under consideration could be reduced to problems in the mathematical theory of permutations and combinations. Upon this basis a convenient calculus was developed, which was subsequently applied to many different fields of inquiry. Huygens, the Bernoullis, Montmort, De Moivre, and Bayes are the most prominent figures in the early history of the subject. Their work was systematized and completed in the great treatise of Laplace (early nineteenth century), and the point of view from which they conducted their analyses remained until quite recently the basis for the interpretation and extension of the mathematical theory. The principle upon which Laplace assigned numerical values to probabilities was that of analyzing the possible outcome of a situation into a set of alternatives which could be judged as "equally possible." Accordingly, although we might be ignorant of which one of these alternatives

would occur, a method was provided by the aid of which an appropriate "degree of rational belief" could be assigned to propositions about "chance events." In brief, fortuitous events which had heretofore been denied the status of genuine objects of scientific knowledge could now be handled in an expert manner with the help of probability theory. The intellectual instrument was thus forged for developing what is now known as the statistical view of nature, and for exhibiting important continuities in techniques and methods in different scientific disciplines.

e) The theoretical foundations of the probability calculus as formulated by Laplace still had their roots in traditional rationalism. On the one hand, probability judgments were understood to betoken ignorance: Laplace maintained that all events are regulated by "the great laws of nature" which a sufficiently powerful intelligence could use to foretell the future in the most minute way. On the other hand, judgments of equipossibility were made to rest on a nonexperimental basis. A critique and reformulation of these foundations were not to come for several decades. Nevertheless, these rationalistic preconceptions were conveniently overlooked in the application of probability theory. One of the earliest and most successful of these applications was to the systematization of measurements and observations in the experimental sciences. Astronomy was the first to employ the theory of probability for this purpose. Justly regarded for a long time as the most exact science of measurement, it nevertheless was patent to everyone that the measurements actually performed did not yield identical numerical values for what was presumably the same magnitude, however carefully gross disturbing factors were eliminated. In consequence, the measurable predictions calculated from astronomical theory were not in precise agreement with the numbers obtained by direct measurement. Given the climate of opinion within which astronomical theory was developed, it was congenial to interpret these fluctuations as deviations or "errors" from the "true values" of magnitudes, and to attribute the "inexactitude" of actual measurements to human failing.

Nevertheless, there was a pressing need for techniques to estimate the "true values" from the actual measurements and to measure the degree to which the latter "approximate" to the former.

This situation is not local to astronomy. As Boyle once explained, "You will meet with several observations and experiments which, though communicated for true by candid authors or undistrusted eyewitnesses, disappoint your expectations, either not at all succeeding constantly or at least varying much from what you expected." Indeed, to test any theory, empirically specified initial conditions must be given, and the consequences logically derived from them with the help of the theory must be compared with the outcome of further observational procedures. Thus, two series of actual measurements or observations must be instituted to test a theory; and, for both series, we find that as a matter of fact there are groups of discordant statements reporting the issue of our measurements. Whether the theory is in accordance with the "facts" cannot therefore be decided without some further hypothesis on the actual measurements we make.

The study of this problem in terms of the theory of probability constitutes what is known as the theory of errors. It was begun in the eighteenth century by Boscovitch, Lambert, Euler, and Thomas Simpson, and was continued by Daniel Bernoulli, Legendre, Gauss, and Laplace. Gauss showed that if we assume that the deviations from the "true magnitude" are produced by a large number of hypothetical "elementary errors" acting independently of one another, the form of the law of distribution of the actual measurements can be deduced, and an approximation to the "true value" can be calculated from the data. The Gaussian "Law of Error" and the Method of Least Squares for systematizing discordant observations have played an important role in subsequent researches in the theory of measurement and statistics. Recent critical work on the foundations of probability shows that Gauss's arguments for the law rest on assumptions which cannot always be made legitimately. In consequence, alternative laws for the distribu-

tion of errors have been proposed, notably by Poisson, Pearson, Gram, and Charlier, each suitable for different circumstances.

f) The expanding national economies following the breakup of the feudal system required the gathering of extensive factual information in order to guide the formulation of financial, military, and political policies. The earliest attempts to tie up the mathematical theory of probability with the analysis of such descriptive statistics were made by De Moivre, Nicholas and Daniel Bernoulli, Euler, and D'Alembert. Under the influence of the ideas of the French Encyclopedists, who sought a rational basis for monetary undertakings, public-health administration, judicial procedure, and even the conduct of elections, Condorcet tried to apply on a comprehensive scale the new mathematical instrument of probability to all such matters. Like Laplace and Poisson after him, he achieved only a modicum of success. It was characteristic of this group of writers to misunderstand and consequently to overrate the function of the probability calculus; their procedure frequently seemed to rest on the assumption, as one commentator remarked, that valuable results can be obtained from unreliable and insufficiently analyzed data by employing a sufficient number of signs of integration. However, it is to the great credit of these men to have insisted on the fusion of statistical methods with the theory of probability. Interest in this fusion was further stimulated by the Belgian astronomer Quetelet, who saw in the theory of probability the appropriate tool for developing a reliable social science. Poisson had enunciated in a somewhat confused form a "law" which he called "the law of great numbers"; according to this law large aggregates of elements exhibit definite properties with a stable relative frequency, even though these properties occur quite fortuitously within the aggregates. Quetelet popularized this idea in the context of the social disciplines. He regarded the "average man," as computed from the extensive statistics he gathered, as the analogue in social matters of the center of gravity in mechanics; and he saw in the statistical regularities with which certain human actions occur the operation of comprehensive laws of social development. He thus

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found it easy to believe that determinate laws could be formulated to connect the different social averages—determinate laws modeled upon those recorded in Laplace's *Celestial Mechanics*. However, Quetelet was uncritical both in gathering his statistical material and in interpreting it; he was never really clear as to the meaning of statistical averages, and never appreciated the limitations of the probability calculus. His influence, great at first, rapidly waned, and for a time so did the interest in applying theoretical statistics to the social sciences.

When interest in the subject was once more revived, it was supported by research needs in biology, psychology, and theoretical physics. Statistical methods subsequently developed on the basis of probability theory were then applied to matters as remote and different as the calculation of the density of telephone traffic and the maintenance of manufactured products at a certain standard of quality. The determination of the character of an indefinitely large population on the basis of samples drawn from it is a problem common to many disciplines and many daily occupations. The elements of an adequate theory of sampling within the framework of a theory of probability were first laid down by Lexis, and further developed by Bortkiewicz, Tschuprow, Markoff, and others. They showed that the sheer number of instances in a sample is no guaranty of its representative character, criticized statistical practice which relied upon the accumulation of unanalyzed numerical data, and developed a technique for obtaining trustworthy statistical coefficients from data grouped carefully according to the variety, homogeneity, and number of the instances. More recently, R. A. Fisher and his school have approached the problem from a different point of view, and, in addition to devising important criteria for the adequacy of statistical coefficients, he has called needed attention to the serious limitations of many of the Laplacian formulas. Other distinct contributions to the theory of sampling have been made by Fechner, Bruns, Galton, Thiele, Pearson, and Neyman. In consequence of these researches, the theory of errors, the theory of sampling, the theory of curve-fitting, now all fall within a comprehensive theory of probability.

g) Although the importance of the main ideas of the mathematical theory of probability for systematizing measurements was quickly recognized in the sciences, the theory of probability was for a long time usually regarded as simply ancillary to the theoretical disciplines. Thus, it was commonly assumed in physics that its laws are statable in "deterministic" form, such that the positions and velocities of elementary particles at one time are connected in precise ways with the positions and velocities at any other time. It is today a commonplace, however, that some of the most fruitful applications of the theory of probability occur *within* the theoretical framework of various sciences. The ancient idea that the apparently permanent objects around us as well as the regularities in their behavior could be viewed as aggregate effects of a large number of hypothetical elements undergoing random changes has frequently attracted the creative minds in science. Thus, Kepler played with it to explain the appearance of a new star in 1604; Boyle had a corpuscular theory for the states of aggregation of bodies; Huygens even formulated a corpuscular theory of gravitation; and Daniel Bernoulli's interpretation of Boyle's law for gases in terms of the kinetic theory of matter is well known.

Apparently, the first man to work out such theories with sufficient quantitative detail to make possible an empirical evaluation of the magnitudes associated with the hypothetical elements was Joule. He computed the average velocities of hydrogen molecules on the basis of statistical considerations and showed that, in order to produce the observed effects, the velocities must lie in specified intervals. The statistical explanation of thermal phenomena was carried to much greater lengths by Maxwell: he showed that, if certain assumptions are made concerning the probabilities with which the particles of a gas acquired different positions and velocities, the familiar gas laws could be deduced. But perhaps the greatest triumph of probability theory within the framework of nineteenth-century physics was Boltzmann's interpretation of the irreversibility of thermal processes; this he was able to do in terms of the most probable distribution of the energies of the molecules of a gas.

In consequence, the second law of thermodynamics can be formulated as a theorem in probability, and irreversible processes turn out to be statistical phenomena.

Thermodynamics is not an isolated instance of the use of statistical concepts within theoretical formulations. Even before Maxwell employed probability theory for the study of gases, G. G. Stokes used it to analyze the effects of polarized light coming from different sources as the average or most probable effects. Again, statistical mechanics, which consistently employs the theorems and the point of view of probability theory, has been fruitfully applied in the study of the history and distribution of the stars. And more recently the entire theory of radiation has been developed to include systematically within itself hitherto unrelated phenomena, on the basis of a profound and radical application of the theory of probability.

But physics is not the only science with has profited from using statistical concepts in its theories. Democritus tried to explain the resemblances and dissimilarities between parents and children in terms of a shuffling of the atoms coming from the ancestors of a child; Aristotle employed related notions in discussing similar problems. And ever since Darwin called attention to the importance of the facts of variation for any adequate biological theory, students of biology have been developing a statistical treatment of the subject. It is obviously essential to distinguish between variations due to heredity and those due to environment; this phase of the subject has been explored by Pearson and his school with the help of the mathematical theory of probability. But attempts such as those of Galton to formulate the laws of heredity in terms of average contributions from the ancestors of a given set of progeny are now known to be unsatisfactory; and Galton's mistakes indicate some of the limitations of statistical methods in general. The theoretical basis for modern experimental genetics was supplied by Mendel. The theory of the mechanism of heredity he proposed, which involved the transmission, segregation, and combinations of unit characters in various proportions, obviously lends itself to be exploited in terms of the fundamental ideas of mathematical

probability; with its help, the artificial selection of plants and animals has been brought to a high stage of perfection. The mathematical theory of natural selection for those groups in which a Mendelian analysis can be made has been worked out mainly by R. A. Fisher, J. B. S. Haldane, and S. Wright.

In general, therefore, the introduction of probability notions into the theoretical structure of physics and biology has been most fruitful. It has made possible the prediction of the relative frequency with which definite characters occur in groups of individuals, even when it is not feasible to predict the occurrence of such characters for a given individual.

h) The developments which have thus far been surveyed have gradually tended to undermine the authority of classic rationalism in science. For, as points of view borrowed from the theory of probability and statistics assume central roles, both within the theoretical framework of the sciences as well as in the procedures of applying theories to matters of fact, it becomes progressively more difficult to assume that the principles of a science are self-evident or necessary. This change in the climate of opinion has been further supported by a general logical criticism of the assumptions of the classic view which began in antiquity. The Epicureans, as well as Skeptics like Carneades, developed conceptions of the logic of inquiry which made allowances for the formally incomplete character of the evidence for empirical statements. In modern times it was Hume's discussion of causality which put the rationalist notion of necessity on the defensive, and since then every variety of empiricism has had its day in court.

Nevertheless, although the Humean analysis was a powerful dissolvent of ancient preconceptions, it was not so powerful as some of the internal technical developments within the special sciences. A few of these have already been indicated. But perhaps the most significant single technical achievement, from the point of view of its general effect upon the philosophy of science, has been the definitive refutation of the thesis that Euclidean geometry is the apodeictic science of space. For the discovery of the non-Euclidean geometries exhibited logically

possible alternatives to conceptions previously regarded as indubitable, while the recognition of a distinction between pure and applied mathematics cut the ground from under the claims of traditional rationalism. Whatever doubt still lingered as to the possibility of alternative conceptions of "space" was finally removed when the Newtonian physics and its Euclidean framework for mechanics were displaced by relativity physics and its framework of Riemannian geometry. And perhaps the final coup de grâce to the claim that physical principles are indubitable and necessary was supplied when the familiar physics of continuous action was found to be inadequate for vast ranges of phenomena, and made way for the contemporary physics of quanta.

There are thus both historical and analytic grounds for the view central to empiricism that there is no a priori knowledge of matters of fact, and there are similar grounds for the thesis of contemporary empiricism that no amount of empirical evidence can establish propositions about matters of fact beyond every possibility of doubt or error. On the other hand, the recognition of this state of affairs raises an important problem. Although our beliefs cannot be established with absolute finality, we do, as we must, differentiate between them on the ground of the character of the evidence which supports them. We regard it more probable that Napoleon was a historical character than that he is a solar myth. We believe that the prognoses of a modern physician are more reliable than those made a century ago. A chemist accepts Lavoisier's theory of combustion as better founded than Stahl's phlogiston theory, and a physicist will urge that the quantum theory of radiation is today more securely based than it was twenty years ago. There is clearly an obvious need for canons to evaluate the evidence supporting any proposition, and for the formulation of the principles we employ in deciding that one statement is better grounded than another. Judging by the success of past attempts to supply them, it may be suspected that every proposed list of such canons and formulations will be incomplete and will require emendation with the progress of inquiry. The need, however,

is a permanent one; and the attempts to satisfy it constitute the broader setting and the larger theme in contemporary discussions of probability.

II. The Calculus of Probability and Its Interpretations

3. Preliminary Distinctions

The vast range of material which has just been outlined has been traditionally regarded as constituting the subject matter for a theory of probability. It has been frequently assumed that a precise meaning can be found for the term 'probable' which is common to each of its use in the contexts indicated. Upon this point competent students are not in agreement. Without prejudging the issues involved, it is possible to distinguish between two groups of statements in which the term 'probable' or its derivatives occur. The first group contains statements such as the following:

'The probability that a man of thirty will survive his thirty-first birthday is .955'; 'The probability that a normal coin will present a head after being tossed is $\frac{1}{2}$'; 'The probability that on the basis of the evidence in 1938 the electronic charge e has a value in the interval $(4.770 \pm .005) \times 10^{-10}$ electrostatic units is .67'; 'The probability that a molecule of hydrogen has a velocity in the interval $v - dv$ and $v + dv$ is p'; 'The probability of a 10° deflection of an a-ray passing through a film is $\frac{1}{4}$'; 'The intensity of a spectral line is determined by the probability of the corresponding quantum transition'; and 'A snowstorm in New York during January is more probable than during November.'

The second group contains such statements as:

'Relative to our present evidence the theory of light quanta has a probability which is greater than its probability relative to the evidence available in 1920'; 'The evidence makes it highly improbable that Aristotle composed all the works attributed to him'; 'The theory of evolution has a higher probability on the evidence than the theory of special creation'; 'It is probable that, had Cleopatra's nose been a half-inch longer, the course

of the Roman Empire would have been different'; and 'It is not probable that Christ was a descendant of King David.'

Statements in the first group employ the term 'probable' in a sense which, by practically unanimous consent, is subject to the rules of the calculus of probability; indeed, the calculus has been explicitly devised to handle such "probabilities." Statements in the second group apparently employ the term to indicate the "degree" of the adequacy of the evidence supporting the proposition; students are not agreed whether the mathematical calculus of probability is applicable to such "probabilities." In the present section we shall for the most part confine ourselves to such statements which clearly fall into the first set; the discussion of the second group of statements is reserved for Section III. But nothing said in the present section will exclude the possibility that both classes of statements, in spite of apparent differences between them, are subject to the same interpretation.

Even though we have restricted the scope of the present section in the indicated way, it is still not possible to specify a sense of 'probable' or even a formulation of the calculus of probability, upon which reasonably complete agreement is obtainable. There are, in fact, three major interpretations of the term. According to the first, a degree of probability measures our subjective expectation or strength of belief, and the calculus of probability is a branch of combinatorial analysis; this is the classical view of the subject, which was held by Laplace and is still professed by many mathematicians. It is not always clear whether by 'expectation' proponents of this view understand *actual* expectations or *reasonable* expectations. According to the second, probability is a unique logical relation between propositions, analogous to the relation of deducibility; its most prominent contemporary supporter is the economist Keynes. According to the third, a degree of probability is the measure of the relative frequency with which a property occurs in a specified class of elements; this view already appears in Aristotle, was proposed by Bolzano and Cournot during the last century and further developed by Ellis, Venn, and Peirce, and was finally

made the basis for a subtle mathematical treatment of the subject by von Mises and other contemporary writers. We shall begin with the exposition of the frequency interpretation of probability and its calculus; subsequently, the other two views will be briefly considered, and it will be argued that the frequency view is the one most suitable for the first of the foregoing two classes of statements; in Section III we shall finally examine some important methodological problems which cluster around the frequency view.[1]

4. Fundamental Ideas of the Frequency Interpretation of Probability

The basic ideas of the frequency conception of probability emerge upon an examination of such a statement as 'The probability that a person of thirty residing in the United States survives his thirty-first birthday is .945.' The meaning of such a statement can be ascertained by examining how it is established. That procedure, greatly simplified, is somewhat as follows. Suppose that during a period of years there is no migration to or from the United States, and that during these years exact counts are made of its inhabitants who fall into definite age groups. Thus, suppose that in 1900 there are 2,000,000 persons who have just reached their thirtieth birthday, and that exactly one year later there are 1,890,000 persons who have just reached their thirty-first birthday; that is, of the thirty-year-olds in 1900 a ratio of .9450 survive at least another year. We imagine that similar figures are obtained for the four succeeding years, and that the ratios of thirty-year-olds who survive their thirty-first birthday are .9452, .9456, .9451, and .9454, respectively. We notice that, although these ratios are not constant, the differences do not appear until the fourth decimal is reached. We may say, therefore, that during these five years approximately 945 out of a thousand thirty-year-old residents of the United States live for at least another year; and we may make the further *assumption* that for an *indefinite number of future years* the corresponding ratios of survivals remain in the neighborhood of .945. Accordingly, the statement 'The probability that a thirty-year-old resident of the United States sur-

vives his thirty-first birthday is .945' means that in the long run the relative frequency with which thirty-year-olds in the United States survive for at least one year is approximately .945.

The following points must be noted in this example. In the first place, the probability statement supplies no information about any *individual* resident of the United States; the information is relevant to the individual Tom Brown only in so far as he belongs to the *class* of thirty-year-old residents. Second, the statement supplies information about no property of this class of residents other than the one explicitly specified, namely, the property of surviving at least one year. Third, the statement supplies a numerical value—the value of a *relative frequency*. Fourth, the statement does not mean that in *every* thousand thirty-year-olds 945 will live for at least another year. And, finally, this numerical value is intended to specify the relative frequency of survivals during an indefinite number of years, or "in the long run," and not only during the years for which an actual count has been made; that is to say, the statement makes a prognosis.

We now turn to the general definition of probability statements. But at once difficulties arise. A proposed definition must be precise and unambiguous and at the same time should be modeled as closely as possible upon the procedures which the foregoing example illustrates. On the other hand, those procedures have been described with the help of terms which are not precise; in particular, the expressions 'in the long run' and 'approximately' are highly vague, and it is not easy to develop a mathematical theory in terms of them. The definition to be proposed will replace these expressions by more precise ones, which are appropriate for developing a *calculus* of probability. Hence, although the definition will be modeled upon the illustration, it will employ precise mathematical concepts to which there cannot easily be assigned a simple empirical meaning. The methodological problems which are a consequence of this procedure will have to be considered subsequently.

Let R be a non-empty class of elements (i.e., it contains at least one member), to be known as the *reference class*; for reasons which will be soon apparent, the elements of R will be supposed to be serially ordered. Let A be some property which the elements of R may exhibit. Suppose R contains n elements, and let $\text{nu}(A \text{ and } R)$ be the number of elements in R which have the property A. We may now define the expression 'the relative frequency with which elements in R have the property A,' which we abbreviate into '$\text{fr}_n(A, R)$,' as follows:

$$\text{'fr}_n(A, R)\text{' is short for } \frac{\text{'nu}(A \text{ and } R)\text{'}}{n}$$

It is evident that a relative frequency is a proper fraction. Suppose that the number of elements in R increases. In general, the fraction $\text{fr}_n(A, R)$ will be different for different values of n. It may happen, however, that these fractions will crowd around some fixed number p, and will differ from it by a small positive magnitude ϵ which diminishes as n increases: in familiar language, $\text{fr}_n(A, R)$ will tend toward p in the long run. The mathematically precise way of rendering this possibility is to say that $\text{fr}_n(A, R)$ approaches p as a limit with increasing n; that is,

$$p = \lim_{n \to \infty} \text{fr}_n(A, R) .$$

What mathematicians understand by 'limit' is illustrated by the following. Consider the infinite series of fractions $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{4}$, $\frac{4}{5}$, ... ; its limit is 1. Suppose we have the infinite series of numbers x_1, x_2, x_3, ... x_n, ... , where the subscripts indicate the ordinal position of the numbers in the series. To say that p is the limit of this series means that, however we may select a positive number ϵ, there is a number N such that for every n, if $n > N$, then the absolute difference between x_n and p (i.e., neglecting signs) is less than ϵ. The reason for requiring R to be serially ordered is now clear. If R contains only a finite number of elements, $\text{fr}_n(A, R)$ is unaffected by the order in which the elements are counted; but the limit of $\text{fr}_n(A, R)$ when R is not finite does depend on the order in which the elements of R (and therefore the relative frequencies) are arranged.

It is very convenient in developing the calculus of probability to define 'probability' as 'the limit of relative frequency.' If we abbreviate statements of the form 'The probability that an

element has the property A if it is a member of R is p' into 'prob$(A, R) = p$,' the definition takes the following form:

$$\text{'prob}(A, R) = p' \text{ is short for } 'p = \lim_{n \to \infty} \text{fr}_n(A, R)'$$

'prob$(A, R) = p$' may be read conveniently as 'the probability of A in R is p.'

Expressions like 'prob(A, R)' which describe numbers are called *numerical expressions* and consist of the *functor* 'prob' together with its arguments; 'nu$(A$ and $R)$' is also a numerical expression, and 'nu' another functor. The expression 'prob(A, R)' is the fundamental numerical expression in the mathematical theory of probability developed on a frequency basis; it describes a real number, which may be irrational, in the interval 0 to 1 inclusive. Within the calculus of probability the statement 'The probability that a thirty-year-old resident of the United States survives his thirty-first birthday is .945' must now be taken as equivalent to 'The limit of the relative frequency with which the property of surviving at least one year occurs in the ordered class of thirty-year-old residents of the United States is .945.'

It has already been pointed out that the foregoing definition of 'probability' has been proposed for the sake of its great convenience in calculations. It employs the notions of infinite ordered classes and of limiting values of relative frequencies in such classes. It is obvious, of course, that in empirical procedures we are occupied with finite classes which may or may not be ordered, and with relative frequencies rather than limits of relative frequencies. Some writers (e.g., Copeland and Popper) have proposed to use as the definition of 'probability' *not* 'the limit of relative frequencies,' but 'the *condensation point* of relative frequencies.' p is said to be a condensation point of the series $x_1, x_2, x_3, \ldots x_n, \ldots$, if, for every positive number ϵ and every N there is an n such that $n > N$ and the absolute value of the difference between x_n and p is less than ϵ. Such a definition has the merit that a proof can be given that there is at least one condensation point for relative frequencies in an infinite reference class, even though none exists; it suffers from the disadvantage that according to it a probability may have more than one probability in a given class, so that the calculus of probability becomes more complicated.[3]

It is essential to note the following points in connection with probability statements interpreted in terms of relative frequencies:

a) No meaning can be attached to any expression which, taken literally, assigns a probability to a single individual as having a specified property. Statements of probability predicate something of an individual (e.g., Tom Brown) only in so far as he is an element in a specified reference class. Probability statements which do not do so *explicitly* must be regarded as incomplete if they are to be significant: they must be understood as making an implicit specification of the reference class within which the designated property occurs with a certain relative frequency.

b) Every probability statement of the form thus far considered is a factual statement, into whose determination empirical investigations of some sort must always enter. Probability statements are on par with statements which specify the density of a substance; they are not formulations of the degree of our ignorance or uncertainty. To assert that the probability of a normal coin presenting head after being tossed is $\frac{1}{2}$, is to ascribe a physical property to a coin which is manifested under determinate conditions.

c) Since probability statements require the specification of a reference class with respect to which a given property has some degree of probability, a given property can be associated with different degrees of probability, according to the reference class which is specified. The probability of surviving at least one year may be .945 with respect to the reference class of thirty-year-old residents of the United States; it may be .734 with respect to the reference class of sixty-year-old men; and it may be .345 with respect to the class of domesticated cats.

d) Since the explicit definition of probability statements is in terms of relative frequencies, the *direct* evidence for them is of a *statistical* nature. Thus, waiving difficulties to be mentioned, the direct evidence for the probability of a coin falling head is obtained by counting the frequency with which it falls head. However, probability statements do not always occur singly and are often part of a more or less inclusive *system* of statements or a *theory*. In such cases the estimation of the numerical values of the probabilities and the subsequent testing

of such values may be made on the basis of *indirect* evidence which in some cases may even be nonstatistical. This point will receive further attention in Section III.

e) Since a probability has been defined as the limit of a relative frequency (or, even more loosely, as the relative frequency in the long run), every probability statement is a hypothesis; such a hypothesis cannot be completely confirmed or finally verified by the (necessarily) finite amount of evidence actually at hand at any given time. It is thus quite possible that the numerical value estimated for a probability on given evidence is not correct, so that revisions of the estimate may have to be made repeatedly. It is partly for this reason that in the history of the subject discussions of probability have run parallel with discussions of the problem of induction. The situation with respect to probability statements is indeed more serious than has been just indicated. For not only cannot probability statements be completely confirmed; they cannot even be completely disconfirmed by any actual evidence. The issues involved will receive further attention below.

f) Finally, it is a mistake to suppose that the successful use of probability statements depends in any way upon the issues of what is popularly known as "determinism." Because current microscopic physics employs theories involving in an essential way probability considerations, many thinkers, including reputable scientists, have been persuaded into supposing that the general breakdown of "mechanistic" explanations has been demonstrated, that processes in nature are "noncausal," and that contemporary physics supplies evidence for the existence of human "freedom" and for a "spiritualistic" world-view. Such suppositions feed upon mistaken or misleading formulations of the actual issues in modern physics, as has been pointed out repeatedly, among others, by Venn, Peirce, Philipp Frank, and Henry Margenau. It is perhaps sufficient to note that the use of probability statements requires no commitment, even by implication, to any wholesale "deterministic" or "indeterministic" world-view; they can be used successfully in such contexts in

which specified properties occur with stable relative frequencies in specified classes of elements.

One of the main difficulties in most debates on causality is that the term is not explained with sufficient precision to make discussion fruitful. (As a matter of fact, specific contributions to the sciences of nature rarely if ever contain the term.) Without entering into detailed analyses of the issues sometimes raised, the following observations may help clarify some of them.

(*i*) Questions of causality can be significantly discussed only if they are directed to the *theories* or *formulations* of a science and not to its subject matter. No clear sense can be given to most pronouncements that the world or any segment of it is a causal process. On the other hand, in discussing the causal or noncausal character of a given theory, two factors must be examined: the *state* (or system of properties) of which the physical system under discussion is described and the *laws* (or system of equations) which connect the states at different times and places. The state of a system is sometimes specified with the help of properties belonging to what are taken as "individual elements," sometimes with the help of the properties of a field, and sometimes in statistical terms involving the properties of aggregates of individuals. The laws also can differ markedly in form: they may establish a unique correspondence between states at different times or they may have the form of probability statements; they may be explicit functions of the time variable or they may not, etc. No universally accepted criterion has been formulated for judging whether a theory is "causal." Classical mechanics is frequently considered as the example par excellence of such a theory; the states considered by it are the positions and momenta of material particles, and its laws are certain differential equations of the second order not containing the time variable explicitly. It is often assumed that, in order to be a causal theory, the states employed by the theory must be those of classical mechanics. In that case, however, neither classical electromagnetics nor modern quantum mechanics are causal theories, although the former is usually so regarded. In some cases, on the other hand, the distinction between noncausal and causal theories is made on the basis of whether the states are specified in statistical terms or not, so that classical statistical mechanics and modern quantum mechanics would both be classified as noncausal theories. The main point to be borne in mind is that both factors, specification of state and form of law, are relevant to the discussion. Even theories which employ statistically specified states have been said to be causal because their laws establish a unique correspondence between its states at different times—although with respect to certain properties of individuals in the system the theories have been classified as noncausal, because the equations supply only probability statements concerning the occurrence of properties of individuals.

(ii) Because probability statements supply no information about any individual member of the reference class, it has been imagined that a physical theory involving probability considerations precludes a "causal" explanation of the phenomena under consideration. Now such a theory will only specify the state in statistical terms; and, as a consequence, the predictions of the theory may have the form of probability statements concerning the properties of individuals. In some cases, however, it is also possible to describe the situation in terms of nonstatistical states, so that laws of a "causal" type may connect these new states. Whether it is possible or convenient to do so is obviously a matter to be decided for each case by experiment and scientific theory. It so happens that for the phenomena studied by classical statistical mechanics it is possible to do this; and, as a consequence, the "indeterminism" of classical statistical mechanics has been usually regarded as eliminable or inessential. Such an elimination is not possible for modern quantum mechanics within the framework of its procedures, and marks an important difference between classical and recent physics. In any case, nothing more than a very technical scientific difference is involved; and at least some physicists are of the opinion that future research may remove this difference. It should also be noted, moreover, that if the Ψ-function in modern quantum mechanics is taken to specify the state of the system, without seeking to interpret this function statistically, quantum theory may also be regarded as a "causal" theory, for its laws have the form of equations usually regarded as of the causal type: they establish a unique correspondence between states at different spatio-temporal regions.[8]

5. Fundamental Theorems in the Calculus of Probability

1. *The function of the calculus.*—It should now be clear that probability statements cannot in general be certified on purely formal grounds, so that pure mathematics and logic are not in the position to assert probability statements of the form considered thus far. What then, it may be asked, are the function and nature of the mathematical calculus of probability? To readers of the preceding monographs in this *Encyclopedia* the answer will be familiar. The calculus of probability has the same general function as a demonstrative geometry or a demonstrative arithmetic: given certain initial probabilities, the calculus of probability makes it possible to calculate the probabilities of certain properties which are related to the initial ones in various ways. Thus, arithmetic cannot tell us how many people live in either China or Japan; but, if the population of China and the population of Japan are given, we can compute the

combined population of these countries. The calculus of probability functions in the same way. It is important to recognize that the propositions asserted *in the calculus* are not factual or empirical statements: they are all certifiable on formal grounds alone, and are analytic of the definitions and rules initially laid down. The proposal to establish the theorems of the calculus of probability by experimentation, which has sometimes been made, is as ill-considered as would be the proposal to prove experimentally that $3^2 + 4^2 = 5^2$. The function of the probability calculus, like that of other calculi, is to make possible the *transformation* of probability statements in order that their theoretical content may be made evident. The calculus thus has an *instrumental function* in the context of empirical investigations. It permits us to derive the relative frequencies with which certain properties occur from initial probability statements which do not explicitly mention those frequencies; in this way the calculus makes possible a more adequate testing of the probability statements which we entertain by making explicit the predictions they involve.

The detailed discussion of the calculus of probability can be undertaken only with the help of the technical apparatus of mathematical analysis. Some familiarity with at least the elementary theorems of the calculus is, however, essential for a just appraisal of its function and limitations. In the present section we shall accordingly state a few standard theorems of the calculus and, incidentally, obtain important material for evaluating the claims of standpoints in the philosophy of science which do not subscribe to an empirical outlook.

2. Elementary theorems of the calculus.—Suppose we wished to obtain the probability that children of white parents are both blue-eyed and blond. The reference class R consists of children born to white parents; the problem requires for its answer the (limit of the) relative frequency with which the properties A (being blue-eyed) and B (being blond) jointly occur in R. This number, prob(A and B, R), could be estimated directly. It may, however, be *calculated* from the following two numbers: the probability of A in R; and the probability of B in the refer-

ence class consisting of blue-eyed children of white parents; i.e., from prob(A, R) and prob(B, A, R). The following theorem, known as the General Product Theorem, can be easily demonstrated: The probability of A and B in R is equal to the probability of A in R, multiplied by the probability of B in A and R. Using familiar mathematical symbolism this can be stated as follows:

$$\text{prob}(A \text{ and } B, R) = \text{prob}(A, R) \times \text{prob}(B, A \text{ and } R) . \quad (1.1)$$

We happen to know that the relative frequency of blond hair in the class of blue-eyed children of white parents is not equal to the relative frequency of blond hair among children of white parents in general. In some cases, however, the probability of B in R does equal the probability of B in the narrower reference class A and R. The properties A and B are then said to be "independent" of each other with respect to R. In such cases we obtain the Special Product Theorem:

$$\text{prob}(A \text{ and } B, R) = \text{prob}(A, R) \times \text{prob}(B, R) . \quad (1.2)$$

Theorem 1.1 may itself be generalized for the joint occurrence of n properties $A_1, A_2, \ldots A_n$.

Suppose now that we required the probability that children of white parents are either blond or black-haired. The properties A (being blond) and B (being black-haired) cannot as a matter of fact jointly occur in the class R; they are said to be "exclusive" with respect to R. For such exclusive properties the following Special Addition Theorem can be easily proved: The probability of A or B in R is equal to the probability of A in R plus the probability of B in R. Again employing mathematical symbolism we obtain:

$$\text{prob}(A \text{ or } B, R) = \text{prob}(A, R) + \text{prob}(B, R) . \quad (2.1)$$

Let us next obtain the probability that children of white parents are either male or female. Properties such as male and female are called "contradictory properties" in the class of human births because they are both exclusive and exhaustive. It is obvious that the probability of being male or female in

the class of children of white parents must be equal to 1. In particular, we can demonstrate the following theorem:

$$\text{prob}(A \text{ or not-}A, R) = 1 , \tag{2.2}$$

and with the help of theorem 2.1 we also obtain

$$\text{prob}(A, R) + \text{prob}(\text{not-}A, R) = 1 . \tag{2.3}$$

Thus if the probability of a male birth among humans is .51, the probability of a female birth in that class must be .49.

Theorems 1.1 and 2.1 are fundamental in the elementary calculus of probability. From them a large number of important consequences can be derived by applying the ordinary rules of logic and arithmetic. A few of them will be mentioned because of their practical and methodological importance.

There is clearly no difference between the probability of A and B in R and the probability of B and A in R. Accordingly,

$$\begin{aligned}
\text{prob}(A \text{ and } B, R) &= \text{prob}(A, R) \times \text{prob}(B, A \text{ and } R) \\
&= \text{prob}(B, R) \times \text{prob}(A, B \text{ and } R) ,
\end{aligned}$$

from which we obtain the Division Theorem:

$$\text{prob}(B, A \text{ and } R) = \frac{\text{prob}(B, R) \times \text{prob}(A, B \text{ and } R)}{\text{prob}(A, R)} , \tag{3.1}$$

which can be given the following more convenient form:

$$\text{prob}(B, A \text{ and } R) = \frac{\text{prob}(B, R) \times \text{prob}(A, B \text{ and } R)}{\text{prob}(B, R) \times \text{prob}(A, B \text{ and } R) + \text{prob}(\text{not-}B, R) \times \text{prob}(A, \text{not-}B \text{ and } R)} . \tag{3.2}$$

Theorem 3.2 is one form of what is known as Bayes's theorem. A more general form is the following: Let $B_1, B_2, \ldots . B_n$ be a set of mutually exclusive and exhaustive properties with respect to R, and let B_i be any one of them. Then

$$\text{prob}(B_i, A \text{ and } R) = \frac{\text{prob}(B_i, R) \times \text{prob}(A, B_i \text{ and } R)}{\sum_{1}^{n} i \, \text{prob}(B_i, R) \times \text{prob}(A, B_i \text{ and } R)} , \tag{3.3}$$

where, as usual, 'Σ' is the sign of summation.

Bayes's theorem and the consequences which have been drawn from it have played important roles in discussions of the foundations of probability, induction, and scientific method. It is therefore important to illustrate how it may be employed, especially since the limitations of its use have not always been clearly understood or remembered. Let R be the very numerous class of shots fired at a certain target; let A be the property of a shot hitting the bull's eye; and, finally, let B_1 be the property of a shot that it is fired from Rifle 1, B_2 from Rifle 2, and B_3 from Rifle 3. All the shots are supposed to be fired from these rifles. The (limiting) relative frequency of shots fired from Rifle 1 is $\frac{2}{3}$, from Rifle 2 is $\frac{1}{6}$, and from Rifle 3 is $\frac{1}{6}$; furthermore, the probability that a shot fired from Rifle 1 hits the bull's eye is $\frac{1}{3}$, while from Rifle 2 it is $\frac{2}{3}$, and from Rifle 3 it is $\frac{1}{2}$. What is the probability that a shot which hits the bull's eye is fired from Rifle 2? The question asks for the value of prob$(B_2, A$ and $R)$; it is obtainable from theorem 3.3 if we remember than $n = 3$, prob$(B_1, R) = \frac{2}{3}$, prob$(B_2, R) = \frac{1}{6}$, prob$(B_3, R) = \frac{1}{6}$, prob$(A, B_1$ and $R) = \frac{1}{3}$, prob$(A, B_2$ and $R) = \frac{2}{3}$, and prob$(A, B_3$ and $R) = \frac{1}{2}$. A simple calculation shows that the required probability is $\frac{2}{7}$.

Bayes's theorem is frequently referred to as a theorem in "inverse probabilities," and it has been traditionally regarded as the instrument for discovering the probability of "causes" or "hypotheses" from known "effects" or "consequences." The reason for this terminology is perhaps evident from the illustration: the probability which is sought is that of the "cause" (namely, of a shot being fired from Rifle 2), on the assumption that certain "effects" have set in (namely, of the shot hitting the bull's eye). But although Bayes's theorem can be demonstrated in the calculus of probability, it can be employed to determine the probability of "causes" only if *all* the probability coefficients in the right-hand side of the formula are given. Of special importance are the probabilities of the form 'prob(B_i, R)' which are sometimes designated as the "antecedent probabilities of the causes." Now it has been often assumed that, if we possess no information to the contrary, these

antecedent probabilities are equal to one another. This assumption has been supported by what is known as the Principle of Indifference. With the help of this principle it has been supposed that probabilities could be determined a priori—that is, without recourse to empirical, and more particularly to statistical, investigations. Consequently, this assumption proceeds from a different conception of probability than the one developed in §4; and for a relative frequency conception of probability the equating of probabilities to one another simply on the ground that we know no reason why they should be unequal is a major error. Proceeding within this different conception of probability, Laplace deduced from Bayes's theorem the so-called Rule of Succession, which for a long time was accepted by eminent thinkers as the basis for reliable scientific predictions. According to this rule, if n events of a certain kind have been observed in succession, then the probability of its recurrence is $(n + 1)/(n + 2)$. Following Laplace, Quetelet declared that, "after having seen the sea rise periodically ten successive times at an interval of about twelve hours and a half, the probability that it will rise again for the eleventh time would be $1\frac{1}{7}$." But it also follows from the rule that, if the tide has not been observed to rise at all, the probability of its rising is $\frac{1}{2}$; and such a consequence is a *reductio ad absurdum* of the rule and of its premisses for any view of probability which defines it in terms of relative frequencies.

In most problems it is not practically or theoretically possible to assign values to the antecedent probabilities in Bayes's theorem which could have any empirical significance. For this reason Bayes's theorem has only a limited use, and few writers today take it seriously as a means for determining the probability of a given hypothesis on the basis of given evidence.

3. *Theorems depending on irregularity in the reference class.*—The theorems which have been mentioned thus far can be demonstrated on the sole assumption that the relative frequency of a property in its reference class has a limit. But many theorems in the calculus which are of greatest importance in practice require that other conditions are satisfied as well.

Suppose that in the class R (e.g., tosses with a coin, where the tosses may be imagined as temporally ordered) the property H (head falling uppermost) occurs as follows, where T (tail falling uppermost) and H are exclusive and exhaustive properties with respect to $R:$

$$H\ T\ H\ T\ H\ T\ H\ T\ H\ T\ H\ T \ldots. \qquad (i)$$

That is to say, we suppose that every other toss yields a head, so that the probability of a coin falling head is $\frac{1}{2}$. Here the property H occurs with an obvious regularity; and, if such were indeed the case for actual throws with a coin, we would very likely not employ probability considerations with respect to it. In fact, however, in actual cases heads and tails occur in no such regular order, but with an irregularity somewhat as follows:

$$HH\ TTT\ HH\ TH\ TT\ HH\ TT\ HHHHH\ TH\ TH\ THH\ TTT \ldots. \qquad (ii)$$

In this finite segment of a hypothetically infinite series the relative frequency of H is .51; but we may imagine that the limiting value of this ratio is also $\frac{1}{2}$. The second series is like the first in having $\frac{1}{2}$ as the limit for the relative frequency of H; it is unlike the first in that H occurs in it irregularly or at random. Various theorems in the calculus of probability depend upon the assumption that the reference classes involved possess such a random character.

It is, however, not easy to give a precise sense to what we mean by 'at random,' and an extensive technical literature now exists which deals with the problems of defining 'irregularity' in a manner suitable for mathematical purposes. The first one to have called attention to the importance of conditions of irregularity and to have worked out systematically a mathematical theory of probability with them in mind is von Mises. His procedure takes its point of departure from the following observation: If in the first of the foregoing series we select the (nonfinite) subseries R' by including in it only the odd terms of R, the probability of H in R' is no longer $\frac{1}{2}$ but is 1. On the other hand, if we select the subseries R' from a random series R (such as the second series above is supposed to be) in the same way as before, the probability of H in R' is still $\frac{1}{2}$; that is, prob$(H, R) =$ prob(H, R'). Now let S be any nonfinite subseries of R, subject to the sole condition that the elements of S

are not selected on the basis of their possessing or not possessing H. If for *every selection* of such a subseries S from R, $\text{prob}(H, R) = \text{prob}(H, S)$, the reference class R is said by von Mises to be irregular. He believes that this definition makes precise our intuitive notion of irregularity and that it formulates the conditions found in games of chance and other fortuitous events. Moreover, he maintains that in order to demonstrate many of the standard theorems in the calculus of probability his condition for irregularity must be assumed. (It is well to bear in mind, however, that considerations such as these which involve infinite classes or classes having certain types of order are pertinent primarily to the *calculus* of probability. They are introduced for the sake of constructing a consistent and powerful instrument of symbolic transformations.)

However, many students have found von Mises' definition unsatisfactory. It can be shown that, if a reference class satisfies von Mises' condition of irregularity, the order in which the specified property occurs in it cannot be formulated by any mathematical function; and doubts have therefore been raised as to the *logical* possibility of a reference class which is to satisfy so stringent a condition of irregularity. Indeed, if the phrase 'every selection of such a subseries S' in the definition is taken seriously, a contradiction can be exhibited in the notion of an irregular reference class. Various attempts have accordingly been made by a number of writers to overcome such difficulties (e.g., by Doerge, Kamke, Tornier, Reichenbach, Popper, Copeland) by distinguishing between different types of irregularity and by proposing conditions of irregularity whose consistency can be established. None of these substitutes, however, is sufficiently strong logically for demonstrating the standard theorems in question in their full generality. But more recently it has been shown by A. Wald that by suitably relativizing the selection of subseries in von Mises' definition to certain very general classes of selections, the logical difficulties can be obviated, while at the same time the consequent restrictions upon those theorems do not seriously impair their general validity.[4]

We shall assume that a mathematically satisfactory definition of irregularity can be given, and proceed to mention a few important theorems which may be demonstrated for reference classes satisfying it. Let R be such a reference class (e.g., throws with a coin) in which the property H (head uppermost) has the probability p while the property T (tail uppermost) has the probability $1 - p$. We now suppose the elements of R to be grouped into sets of n successive elements each, and ask for the probability that exactly r elements in a set (where $r \leq n$) have the property H while the remaining $n - r$ elements have

T. The numerical value of this probability can be shown to be equal to

$$\frac{n!}{r!(n-r)!} \, p^r(1-p)^{n-r} \qquad (4.1)$$

where $r! = 1 \times 2 \times 3 \times \ldots \times (r-1) \times r$, with $0! = 1$.

It is of some importance to understand clearly what this number signifies. Suppose that H and T occur in R as in series (ii) on page 32 above, and suppose that R is broken up into sets of four successive elements each. The following sequence of sets results:

$$(HHTT) \quad (HTTT) \quad (TTTH) \quad (TTHH) \quad (THHT)$$
$$(HHTH) \quad (HTHT) \quad (THTT) \ldots \qquad \text{(iii)}$$

Some of these sets, such as the first and second, are overlapping, in the sense that they contain common terms from R; others, such as the first and fifth, are nonoverlapping. If we let $r = 1$, the number given by theorem 4.1 is the limit of the relative frequency with which these sets contain one H and three T's; that is, $4p(1-p)^3$.

Suppose now that $p = \frac{1}{2}$. The probability that in sets of four successive elements from R there is just one element with the property H and three with T, is then $\frac{1}{4}$. But the probability that in such sets there are just two heads and two tails (here $r = 2$) is $\frac{3}{8}$. Hence, when n and p are fixed, the number determined by theorem 4.1 will vary with r. What value of r will yield a maximum value for this number? It can be shown that r must satisfy the condition

$$pn + p \geq r \geq pn + p - 1. \qquad (4.2)$$

When n is very large, the value for which r yields a maximum may be taken to be pn. This means that the probability of sets with n successive elements containing just r elements with the property H is a *maximum*, when r is approximately equal to pn; that is to say, the most probable value occurs for the case when the relative frequency of H in a set of n elements is approximately equal to the *limit* of the relative frequency of H in R.

A very important consequence, known as Bernoulli's theorem, can now be derived, which plays a central role in the practical use of the probability calculus. It can be stated as follows:

Let R be a reference class which is irregular with respect to a property H, and let $\mathrm{prob}(H, R) = p$. Let R be broken up into sets of n successive elements each, and let ϵ be any positive number no matter how small. The probability that H will occur in these sets with *frequencies* lying in the interval $pn \pm \epsilon n$ (or with *relative frequencies* lying in the interval $p \pm \epsilon$) approaches 1 as a limit as n increases (4.3).

The following will illustrate the theorem: R is the irregular class of throws with a coin, and the probability of getting a head is taken to be $\frac{1}{2}$. We ask for the probability that in sets of n successive throws each, the frequency of heads will differ from $n/2$ by not more than $n/10$ (or that the relative frequency of heads will differ from $\frac{1}{2}$ by not more than $\frac{1}{10}$). According to Bernoulli's theorem, this probability tends to 1 as the value of n is increased. Thus, the probability that in ten successive throws there will be anywhere from four to six heads (i.e., that the relative frequency of heads will lie in the interval $\frac{1}{2} \pm \frac{1}{10}$ or $\frac{4}{10}$ to $\frac{6}{10}$) is .47; the probability that in thirty successive throws there will be anywhere from twelve to eighteen heads is .73; the probability that in fifty throws there will be anywhere from twenty to thirty heads is .84; the probability that in one hundred successive throws there will be anywhere from forty to sixty heads is .95; the probability that in five hundred throws there will be anywhere from two hundred to three hundred heads is .99, etc.

These numerical values are calculated with the help of mathematical techniques explained in treatises on probability. Of particular importance in the application of the probability calculus is the analytic formula

$$\phi(t) = \frac{2}{\sqrt{\pi}} \int_0^t e^{-x^2} dx\,,$$

which is obtained from theorem 4.1 by a series of approximations. $\phi(t)$ is the probability that in sets of n successive elements of R which is irregular with respect to H, H occurs with a frequency lying in the interval

$$pn \pm t \sqrt{2p(1-p)n}\,,$$

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where prob$(H, R) = p$; and tables of values of '$\phi(t)$' for different values of 't' have been constructed. In many problems the elements of R are assumed to take on any one of an infinite set of properties. Thus, in measuring the length of objects we may suppose that the measurements are carried out with great precision, and we may accordingly find it convenient to assume that the possible values of the length are real numbers (in the strict mathematical sense). The problems arising in such cases lead to the theory of continuous or geometrical probability.

The theorem of Bernoulli has been generalized by Poisson, and more recently Cantelli and Polya have given an important extension for it. A more general theorem than that of Bernoulli has been established by Tchebycheff, which was further elaborated by Markoff.

There is also an inverse of Bernoulli's theorem, which is sometimes referred to as Bayes's theorem and is obtained with the help of theorem 3.3; it has played an important part in the theory of statistics. But this theorem in inverse probability, with whose help the probability of a statistical hypothesis is to be established on the basis of the samples that have been drawn, suffers from the serious limitations and difficulties already pointed out in connection with theorem 3.3. Critical statisticians no longer make use of it. Statisticians have now developed more suitable procedures for handling the sort of problems Bayes's theorem was intended to solve; the method of maximum likelihood, recently proposed by R. A. Fisher, is a valuable and interesting contribution to this phase of theoretical statistics.

As already indicated, many theorems of the probability calculus are demonstrable only on the assumption that the reference classes are irregular—or in easily understood intuitive terms, that there is a general independence between the occurrence of a property on one occasion and its occurrence on another. However, in many fields of research (e.g., the behavior of gases) such independence cannot, on physical grounds, be assumed to exist. Nonetheless, it has been shown that the calculus of probability may be applied even to such domains with consistency and success.[b]

It is worth while mentioning a seemingly fatal criticism of the definition of 'probability' as 'the limit of relative frequencies.' Let R be irregular with respect to H, and let $f_1, f_2, \ldots, f_n, \ldots$, be the series of relative frequencies of H in R after the first, second, nth terms. (Thus, in the series [ii] of p. 32, the relative frequencies are: $\frac{1}{2}, \frac{2}{3}, \frac{2}{4}, \frac{3}{5}, \frac{3}{6}, \frac{3}{7}, \ldots$) Suppose that p is the limit of these frequencies. Then, once a number ϵ has been selected, there must be an N such that for every n greater than N the difference between f_n and p is less than ϵ; and this means that after the Nth term in R, the relative frequency of H in R will have to remain close to p. But according to theorem 4.1 there is a probability, which though small is not zero, that a very long run of successive H's will occur; and, according to the criticism being considered, there is this probability that even after the Nth term in R such a long run of

H's will set in. However, the criticism continues, a sufficiently long run of *H*'s will make some of the f_n's (with $n > N$) differ from p by *more than* ϵ. A contradiction is thus alleged in the calculus of probability developed on a limit basis. (Thus, suppose $p = .50$, $\epsilon = .01$, and that *N* is taken to be 100; and suppose that beginning with the one hundred and first throw a run of two hundred heads sets in. If *x* is the number of heads which have appeared in the first one hundred throws, $f_{300} = (x + 200)/300$ which differs from $.50$ by more than $.01$.)

However, the allegation of contradiction itself rests on a blunder and proceeds from a conception of probability according to which it is significant to ascribe a probability to a single occurrence. The probability specified by theorem 4.1 does *not* permit us to infer a long run of *H*'s starting with an *assigned* term in *R*, for example, the $N + 1$th. That probability has for its reference class R^*, the class having as *its* elements *sets* of *n* successive elements from *R*; while *p* has *R* for its reference class. (It is possible and significant to ask for the probability that a definite run of *H*'s begins at some assigned term; but the answer to it is *not* given by theorem 4.1. Such a question involves the consideration of a *series* of reference classes such as *R*. An examination of this more complicated problem shows that the objection being considered confuses convergence in *R* with *uniform convergence* in a series of *R*'s.) There is thus no incompatibility between the statement that there is a nonvanishing probability of *H* occurring with a relative frequency *different* from *p*, in sets of *n* elements each (here the reference class is R^*); and the statement that the probability of *H* occurring in *R* is *equal* to *p*. It is true, of course, that in assigning a certain value to *N* we may be committing an error, because for a time the relative frequencies of *H* may diverge from *p*. But this does not establish a *contradiction* in the limit definition of probability; it simply testifies to the difficulty in fixing a value for *N*. That definition does not supply us with an effective method for obtaining a value for *N* either by calculation or in some other way; it merely asserts the existence of such a number *N*. For this reason it has been subjected to various criticisms by finitists, some of which will be considered below.[8]

4. Formalization of the calculus of probability.—Two points should be noted in the foregoing presentation of theorems in the calculus. In the first place, the theorems were formulated and explained in terms of an explicit definition of 'probability' as 'the limit of relative frequencies.' And, second, no primitive propositions were specified from which the theorems of the calculus may be derived with the help of the rules of logic. From the standpoint of a formal mathematical discussion, as well as from the point of view of modern methodology, these are defects, and they require a brief discussion.

If the functor 'prob' is introduced as the defined equivalent of 'the limit of relative frequencies,' every proposition of the calculus is simply a transcription of a theorem in the theory of limits; and every proposition is an analytic statement which can be certified on formal grounds alone. When the calculus is developed in this way, there is no need to supply a special set of primitive sentences: the primitive sentences sufficient for the theory of real numbers are also sufficient to establish every theorem in the calculus.

However, while it may be an advantage to have every theorem of the calculus an analytic sentence of arithmetic, the frequency interpretation of the functor 'prob' is not the only one that is possible. The state of affairs here is strictly analogous to what obtains in geometry. As geometry is employed in physics, the terms 'point,' 'line,' 'plane,' etc., which occur in Euclid, designate certain physical configurations; consequently, the propositions of geometry (such as that the angle-sum of a triangle equals two right angles) formulate measurable relations between physical configurations in exactly the same way as do the propositions of mechanics. But the derivation of geometric theorems from the primitive propositions of Euclid does not depend upon the correlations which happen to be established between terms like 'point' and 'line' and determinate physical configurations. Indeed, formal or demonstrative geometry is not a branch of physics: its theorems cannot be significantly characterized as empirically true or false, because the nonlogical terms in them (e.g., 'point') are uninterpreted. Only after *semantical rules* have been introduced (sometimes also called *coordinating definitions*), which correlate such uninterpreted terms with terms employed to designate empirical subject matter, is a formal geometry transformed into a part of natural science. By distinguishing between pure and physical geometry, not only do we avoid confusing questions of formal validity with questions of empirical fact but we also increase the applicability of pure geometry. Alternative co-ordinating definitions may be introduced, so that qualitatively different subject matters may be explored in terms of the same formal system. On the other

hand, we may also find that, of the many distinct pure geometries which are logically possible, one system is a more effective means than another for organizing the materials of an empirical subject matter.

Similarly, it is not necessary to interpret 'probability' in terms of frequencies in order to develop a formal calculus of probability. The formalization of the probability calculus is of special importance because of the conflicting interpretations which have been given to the term 'probable,' as well as because of the wide range of opinion concerning the conditions under which probability statements are to be regarded as significant. As in the case of geometry, the probability calculus can be formalized in different ways, depending on what terms are selected as primitive, on the mathematical apparatus which is to be employed in developing it, and also upon the use to which it is to be put subsequently. Only one condition is usually observed in formalizing the calculus of probability: it is required that theorems which have been traditionally regarded as standard ones in the subject (such as the addition theorem or Bernoulli's theorem) be derivable from the primitives of the system.

Only a brief mention is here possible of some of the points of view from which the calculus may be formalized. To understand some of them, the distinctions (made in Vol. I, No. 3) will have to be recalled between the language of a science itself, the syntax language whose object-language is the language of science, and the semantic language of the language of science. Statements in the first language refer to what is commonly called the subject matter of the science, statements in the second refer to the order and possible arrangements of the expressions in the object-language, while statements in the third refer to the relations between an expression in the object-language and its subject matter. One difference between probability calculi arises from the fact that probability statements have been formulated in each of these three languages; another difference is due to the fact that some probability statements are metricized while others are not; and a third difference is due to several attempts to incorporate the probability calculus into

a general logic which would include both necessary and probable inference.

Two broad classes of calculi of probability may be distinguished: those which provide a metric for the fundamental functor 'prob' and those which do not. Nonmetrical probability calculi may be further distinguished according as they introduce a definite serial order for probabilities or not. The motivation for the construction of nonmetrical calculi has usually been the desire to interpret probability statements in a nonfrequency sense. Such interpretations are often used by writers who have their eyes on the problems of induction and the estimation of evidence in history and legal procedures. A nonmetrical calculus has been developed by Keynes, but the subject is still in a very unsatisfactory and primitive state.

It is possible to formulate a frequency theory of probability both in the object-language and in the semantic language of a science, the choice between these alternatives being largely a matter of convenience. The probability statements of physics occur in its object-language, and most writers who approach the problems of probability from the natural sciences prefer an object-language formulation. Calculi in the object-language usually associate a number p with a probability, such that $0 \leq p \leq 1$. Some writers restrict the values of 'p' to rational numbers; others permit it to vary in the field of real numbers. A formalized calculus may be developed by taking 'prob(A, R)' as an uninterpreted two-place numerical expression; the logical properties of the expression are then determined by a set of postulates from which, with the help of the usual rules of logic, the standard theorems may be derived. These postulates are abstract in the sense that no restrictions are imposed on the possible interpretations of the functor other than the trivial one that every such interpretation satisfy these postulates. Abstract sets of postulates for probability have been given by Borel, Cantelli, Kolmogoroff, Popper, Reichenbach, and several other writers. (It is also possible to formalize the calculus by taking a one-place numerical functor as primitive, and subse-

quently defining a two-place functor in its terms; and there are other possibilities as well.) From an abstract mathematical point of view, the probability calculus is a chapter in the general theory of measurable functions, so that the mathematical theory of probability is intimately allied with abstract point-set theory. This aspect of the subject is under active investigation and has been especially cultivated by Borel, Fréchet, and a large number of French, Italian, Polish, and Russian mathematicians. In object-language calculi, the arguments '*A*' and '*R*' to the numerical expression 'prob(*A, R*)' are usually predicates or predicate variables; in semantic and syntactical calculi the arguments are usually names of sentences or variable designations of sentences. Postulates for metricized semantic calculi are similar to metricized object-language calculi except for the difference in the kind of arguments the functors take. Such semantic postulates have been given by Mazurkiewicz, Popper, and others.[7]

The possibility of interpreting a formal calculus of probability in different ways can be illustrated by the following list: (i) The functor 'prob' may be interpreted as the limit of relative frequencies in an infinite reference class; the postulates are then transformed into analytic propositions in the theory of real numbers. (ii) The functor may be defined as a relative frequency in a finite reference class; some of the postulates then become analytic propositions in the elementary arithmetic of rational numbers, while others must be suppressed. (iii) The functor may be interpreted, as in the classical Laplacian formulation, as the ratio of the cardinality of two sets of alternatives; the postulates are again converted into analytic propositions in elementary arithmetic. (iv) The functor may be interpreted, as by F. P. Ramsey, as a measure of "partial beliefs," where a degree of probability is a measure of the extent to which a man is prepared to act on a belief. (v) The functor may be interpreted as the ratio of two areas; the postulates become statements in some system of geometry. (vi) A proposal has been made by C. G. Hempel to introduce co-ordinating definitions

for the functor in such a way that, while it will refer to relative frequencies in a class, the postulates are converted into synthetic statements of physics. (vii) A semantical interpretation has been give to the functor (Reichenbach), according to which it designates the truth-frequency of sentences in certain ordered classes of sentences. (viii) According to another interpretation, which also appears to be semantical, the functor denotes the "degree of falsifiability" of a theory (K. Popper). (ix) The functor has been interpreted as referring to the degree of a unique relation between a "proposition" and a set of premises. (It is not clear, however, how this view is to be understood. The language in which it is proposed sometimes suggests that the relation is a syntactical one holding between sentences, sometimes that it holds between the "possible facts" which the sentences designate, and sometimes that it is a semantical relation.) Some writers who take this interpretation do not regard the functor as a numerical one (Keynes), while others explicitly do so (H. Jeffreys).[8]

There is another standpoint from which the formalization of the calculus has been undertaken. Leibniz was one of the earliest writers to broach the possibility of a general formal logic in which the calculus of probability would occupy a central place. According to such a project, the standard relations of deducibility between propositions are to be regarded as limiting cases of a more inclusive relation of "probability implication." Many writers after Leibniz, including Boole and Peirce, kept the ideal of such a general logic a live one; and Clerk-Maxwell went to the extent of declaring that "the true logic for this world is the calculus of probability." However, little was done to actualize this possibility until the very recent development of polyvalent logical calculi. The fusion of familiar formal logic and the calculus of probability into one compendent formal system is now actively investigated. But, although much important work has been already done, there is at present still no satisfactory system of such a general logic.

The calculi of n-valued logics of sentences (with n a finite integer) were first developed by J. Lukasiewicz and E. Post. These calculi reduce to the

standard sentential calculus (e.g., of *Principia mathematica*) when $n = 2$; that is, when a sentence is permitted to take just two "truth-values," namely, truth and falsity. There are certain partial analogies between the theorems of polyvalent logics and theorems of the probability calculus when the latter is suitably formulated; and a number of writers, including Mazurkiewicz, Reichenbach, and Zawirski, have been exploiting these analogies, with the intent of formalizing the calculus of probability as a polyvalent sentential calculus. Reichenbach's method, stated in outline, consists in interpreting an infinite-valued sentential calculus (with values lying in the interval 0 to 1 inclusive) so that each truth-value is the limit of the relative frequency with which the members of definite sequences of propositions are true. He has urged, moreover, that such an infinite-valued "probability logic" is the one most appropriate for science—on the ground that no empirical statement can be completely verified and can therefore be associated with a "truth-value" which in general is different from 0 (falsity) and 1 (truth). Reichenbach's proposal is not free from technical difficulties, and most students are not convinced that he has achieved a fusion of the probability calculus and a general logic of propositions. For example, Reichenbach's polyvalent "probability logic" contains expressions which apparently are subject to the rules of the ordinary two-valued logic; and it therefore seems that his probability logic is constructed upon a basic two-valued schema. Again, his probability logic is nonextensional, in the strict sense of this term in standard use, while the general system of logic commonly employed in mathematics and physics is extensional; it is therefore not easy to see how the latter can be a specialization of the former.[9]

Interest in the fusion of formal logic and the calculus of probability into one comprehensive system has also been exhibited by physicists impressed by the part which probability statements play in modern quantum theory. In that theory certain noncommutative operators occur, as a consequence of some of the fundamental physical assumptions of the theory; and it is possible to regard such operators as a species of logical multiplication upon propositions dealing with subatomic phenomena. However, instead of superimposing such noncommutative multiplications upon the general framework of a logic of propositions in which multiplication (i.e., and-connection) is commutative, proposals have been made to revise the general logic of propositions. According to some of these proposals, a multiplication which is noncommutative will be governed by the formal rules of the logic of propositions and will not be introduced simply as a consequence of a special physical theory. Attempts to re-write quantum mechanics upon the basis of an altered sentential calculus have been made by J. von Neumann, G. Birkhoff, M. Strauss, and others. But researches in this field have not yet gone far enough to permit a judgment on the feasibility and convenience of the proposed emendations.[10]

6. Nonfrequency Interpretations of Probability Statements

We must now briefly consider other interpretations of probability statements than the frequency view proposed earlier in the present section.

1. *The classic conception of probability.*—As already noted, the mathematical theory of probability was first developed in connection with games of chance, and the point of view from which it was cultivated received its classic formulation in the treatise of Laplace. According to the Laplacian view, all our knowledge has a probable character, simply because we lack the requisite skill and information to forecast the future and know the past accurately. A degree of probability is therefore a measure of the amount of certainty associated with a belief: "I consider the word *probability*," De Morgan explained, "as meaning the state of the mind with respect to an assertion, a coming event, or any other matter on which absolute knowledge does not exist." What is required for a mathematical treatment of probability, however, is an exact statement of how this measure is defined; and the classical account is as follows.

Judgments of probability are a function of our partial ignorance and our partial knowledge. We may know that in a given situation the process studied will have an issue which will exhibit one out of a definite number of alternative properties; thus, in tossing a die any one of the six faces may turn up. (These alternative properties have been called the "possible events.") On the other hand, we may have no reason to suppose that one of these events will be realized rather than another, so that, as Laplace remarked, "in this state of indecision it is impossible for us to announce their occurrence with certainty." But a measure of the appropriate degree of belief in a specific outcome of the process can be obtained. We need simply analyze the possible outcome into a set of "equipossible alternatives," and then count the number of alternatives which are favorable to the event whose probability is sought. This measure, the probability of the event, is a fraction whose numerator is the number of favorable alternatives, and whose de-

nominator is the total number of possible alternatives, provided that all the alternatives in question are equipossible. Thus, the probability of obtaining six points with a pair of dice is $\frac{5}{36}$, because the dice can fall in any one of thirty-six equally possible ways, five of which are favorable to the occurrence of six points in all. On the basis of this definition, the probability calculus was developed as an application of the theory of permutations and combinations.

Almost all writers on probability in the nineteenth century (e.g., Poisson, Quetelet, De Morgan, Boole, Stumpf), and many contemporary mathematicians (e.g., Borel, De Finetti, Cantelli, Castelnuovo), follow Laplace with only relatively minor variations. Because of its historical role, as well as because of its contemporary influence, we shall briefly examine this view.[11]

a) According to the Laplacian definition, a probability statement can be made only in such cases as are analyzable into a set of equipossible alternatives. But, while in some cases it seems possible to do this, in most cases where probability statements are made this is not possible. Thus suppose that a biased coin is assigned the probability of .63 that it presents a head when tossed; there is no clear way in which this number can be interpreted as the ratio of equipossible alternatives. This is perhaps even more evident for statements like 'The probability that a thirty-year-old man will live at least another year is .945.' It is absurd to interpret such a statement as meaning that there are a thousand possible eventuations to a man's career, 945 of which are favorable to his surviving at least another year. Moreover, the Laplacian definition requires a probability coefficient to be a rational number. But irrational numbers frequently occur as values for such coefficients, and there is no way of interpreting them as ratios of a number of alternatives. Thus, on the basis of certain assumptions, it can be calculated that the probability that two integers picked at random are relatively prime is $6/\pi^2$. This number cannot be made to mean that there are π^2 equally possible ways in which pairs of integers can be picked, six of which are favorable to getting relative primes.

b) Writers on the subject have not always been clear as to whether they regarded a probability as the measure of a (psychological) belief, or whether they regarded it as a measure of the degree of belief one *ought* to entertain as reasonable. If a probability coefficient is the measure of a degree of actual certainty or the strength of a belief, the addition and multiplication of probabilities require that we determine procedures for combining certainties or beliefs in some corresponding manner. There are, however, no known methods for adding beliefs to one another, and indeed it is difficult to know what could be meant by saying that beliefs are additive. The proposals of Ramsey and De Finetti, to measure strength of beliefs by the relative size of the bets a man is willing to place, are based on a dubious psychological theory; and at least Ramsey's proposal leads directly to a definition of probability in terms of relative frequencies of actions. On the other hand, if probability is a measure of the amount of confidence one ought to have in a given situation, the Laplacian view offers no explanation of the source of the imperative. It is possible, finally, that a probability coefficient is simply a conventional measure of a degree of belief; in that case, however, probability statements turn out to be bare tautologies.

c) According to the Laplacian definition, the alternatives counted must be equally possible. But if 'equipossible' is synonymous with 'equiprobable,' the definition is circular, unless 'equiprobable' can be defined independently of 'probable.' To meet this difficulty, a rule known as the Principle of Indifference (also as the Principle of Insufficient Reason and as the Principle of the Equal Distribution of Ignorance) has been invoked for deciding when alternatives are to be regarded as equiprobable. According to one standard formulation of the rule, two events are equiprobable if there is no known reason for supposing that one of them will occur rather than the other.

It can be shown, however, that, when this form of the rule is applied, incompatible numerical values can be strictly deduced for the probability of an event. An emended form of the rule has been therefore proposed, according to which our *relevant*

evidence must be *symmetrical* with respect to the alternatives, which must not, moreover, be divisible into further alternatives on the given evidence. This formulation seriously restricts the application of the Principle of Indifference. Apart from this, however, two points should be noted: A coin which is known to be symmetrically constructed (so that according to the principle its two faces are to be judged as equiprobable) may nevertheless present the head more frequently than the tail on being tossed; for the relative frequency of heads is a function not only of the physical construction of the coin, but also of the conditions under which it is tossed. Second, no evidence is perfectly symmetrical with respect to a set of alternatives. Thus, the two faces of a coin are differently marked, they do not lie symmetrically with respect to the earth's center at the instant before the coin rises into the air, etc. The emended rule therefore provides that it is only the relevant evidence which is to be considered. But if 'relevance' is defined in terms of 'probable,' the circle in the Laplacian definition is once more patent; while, if judgments of relevance are based on definite empirical knowledge, the ground is cut from under the basic assumption of the Laplacian point of view.

d) It is usually assumed that the ratio of the number of favorable alternatives to the number of possible ones (all being equipossible) is also a clue to the relative frequency with which an event occurs. There is, however, no obvious connection between the 'probability of obtaining a head on tossing a coin' as defined on the classical view, and 'the relative frequency with which heads turn up.' For there is in fact no *logical* relation between *the number of alternative ways* in which a coin can fall and *the frequency* with which these alternatives in fact occur. It has, however, often been supposed that Bernoulli's theorem demonstrates such a connection. For as already explained, according to that theorem if the probability of head is $\frac{1}{2}$, then the probability approaches 1 that in n tosses there are approximately $n/2$ heads as n increases. But the supposition that Bernoulli's theorem establishes a relation between a priori (i.e., determined in accordance with the classical definition) and a

posteriori probabilities (i.e., determined on the basis of relative frequencies of occurrence) is a serious error. It commits those who make it to a form of a priori rationalism. For within the framework of the classical interpretation of the calculus, Bernoulli's theorem simply specifies the relative number of certain types of equiprobable alternatives, each consisting of *n* tosses; it is no more than a theorem in arithmetic and does *not* permit us to conclude that these alternatives will *occur equally often.* That is to say, only if the expression 'The probability of heads is $\frac{1}{2}$' designates a relative frequency of occurrence, can the phrase 'The probability approaches 1' be legitimately interpreted as designating relative frequencies of occurrences.

2. *Probability as a unique logical relation.*—A number of modern writers, conscious of the difficulties in the classical view of probability as a measure of strength of belief, have advanced the view that probability is an objective logical relation between propositions analogous to the relation of deducibility or entailment. According to this version, a degree of probability measures what is often called "the logical distance" between a conclusion and its premisses. The evaluation of a degree of probability therefore depends upon recognizing the inclusion, exclusion, or overlapping of logical ranges of possible facts. Though varying considerably among themselves, something like this view (which has had its forerunners in Leibniz and Bolzano) is central to von Kries, Keynes, J. Nicod, F. Waismann, and several other writers. Only the standpoint of Keynes will be examined here.[12]

For Keynes, probability is a unique, unanalyzable relation between two propositions. No proposition as such is probable; it has a degree of probability only with respect to specified evidence. This relation of probability is not a degree of subjective expectation; on the contrary, it is only when we have perceived this relation between evidence and conclusion that we can attach some degree of "rational belief" to the latter. (As already noted, Keynes's formulation of his view is not unambiguous. His occasional language to the contrary notwith-

standing, it does not seem likely that he regards his probability relation as a syntactical one. The present writer is inclined to the opinion that it is a semantical relation.) It is characteristic of Keynes's standpoint that the *secondary* proposition, which asserts that a proposition p has the probability relation of degree a to the proposition h, can and must be known to be true "with the highest degree of rational certainty." Such a highest degree of rational certainty is obtainable, according to Keynes, when we see that the conclusion of a syllogism follows from its premisses, as well as when we see that a conclusion "nearly follows" from its premisses with degree a of probability. However, degrees of probability are not in general capable of measurement; indeed, according to Keynes, probabilities cannot in general be even ordered serially, although in some cases they are comparable. The comparison of probabilities, whenever this is possible, is effected with the help of the modified Principle of Indifference mentioned above; and the judgments of relevance which the principle presupposes are themselves direct judgments of degrees of probability. In terms of such an apparatus of concepts, Keynes develops a calculus which formulates the relations between comparable probabilities, and finally explains how and under what limited circumstances numerical values may be assigned to degrees of probability.

Although Keynes avoids some of the difficulties of the classical view of probability, his general standpoint has difficulties of its own. Omitting all discussion of the technical difficulties in his calculus, we shall confine ourselves to a brief mention of three central issues.

a) On Keynes's view we must have a "logical intuition" of the probable relations between propositions. However, few if any students can be found who claim for themselves such an intuitive power; and no way has been proposed to check and control the alleged deliverances of such direct perceptions in cases where students claim it. Moreover, the possession or lack of this power is wholly irrelevant in the actual estimation of probabilities by the various sciences. No physicist will seriously

propose to decide whether two quantum transitions are equiprobable by appealing to a direct perception of probability relations; and, as N. R. Campbell remarked, "anyone who proposed to attribute to the chances of a given deflection of an α-ray in passing through a given film any sense other than that determined by frequency could convince us of nothing but his ignorance of physics."

b) Since on Keynes's view numerical probabilities can be introduced only when equiprobable alternatives are present, he cannot account for the use of numerical probabilities when such an analysis is not possible. Moreover, like the classic interpretation, Keynes cannot establish any connection between numerical probabilities and relative frequencies of occurrences. His theory, when strictly interpreted, is incapable of application to the problems discussed in physics and statistics, and at least from this point of view remains a vestal virgin.

c) On Keynes's view it is significant to assign a probability, with respect to given evidence, to a proposition dealing with a single occasion. For example, it is permissible to declare that on given evidence the probability of a given coin falling head uppermost *on the next toss* is $\frac{1}{2}$. However, the coin, after it is thrown and comes to rest, will show a head or it will show a tail; and no matter what the issue of the given throw is, the probability of obtaining a head on the initial evidence is and remains $\frac{1}{2}$. No empirical evidence is therefore relevant either for the confirmation or for the disconfirmation of that probability judgment, unless we invoke indirectly a relative frequency in a group of statements—which would be contrary to Keynes's intent. But this is to fly in the face of every rule of sound scientific procedure. A conception of probability according to which we cannot in principle control by experiment and observation the probability statements we make is not a conception which recommends itself as germane to scientific inquiry.

Except for matters to be discussed in Section III, the difficulties which have been pointed out for the classical and the logical interpretations of probability do not embarrass the fre-

quency view. For this negative reason, but especially because it is in accord with scientific practice, the frequency interpretation of probability is the one most suitable for the first class of statements which was specified at the beginning of this section.

III. Unsettled Problems of General Methodology

7. Logical Problems of the Frequency Interpretation of the Probability Calculus

It was shown in Section II that the definition of 'probability' as 'the limit of relative frequency' is *suggested* by common practice in assigning probability coefficients. But it has also been argued that a probability of $\frac{1}{2}$ for head turning up when a coin is tossed means, roughly, that in half the cases of flipping a coin the head is presented. However, such a statement does not mean that in every two tosses a head turns up just once, for in that case it would be absurd to apply it to an odd number of throws; and we would not regard the statement as erroneous if, after getting a tail, we did not get a head on the next succeeding throw. Accordingly, a less misleading explanation of what a probability of $\frac{1}{2}$ signifies is that in a long run of throws the relative frequency of heads is approximately $\frac{1}{2}$. But it has also been pointed out that a definition of 'probability' as 'the approximate ratio of frequencies in the long run' is not precise and is not suitable for mathematical purposes. A definition in terms of limits, on the other hand, has the requisite precision, and a logically consistent calculus can be developed on such a basis. The convenience and fruitfulness of such a definition for the purposes of a calculus of probability are indeed beyond question.

However, from the point of view of the application of the calculus to empirical matters, it would be of little profit to have a precise mathematical definition of 'probability' if as a consequence every probability statement would acquire a theoretical content which cannot be controlled by acknowledged empirical methods. But an examination of the form of probability statements, when these are interpreted in terms of limits of relative frequencies, seems to indicate that such is indeed the

case. This may be seen concretely in the following way. Suppose we test the hypothesis that the probability of heads is $\frac{1}{2}$ by flipping the coin a thousand times, and suppose we get a run of a thousand heads. We might be inclined to conclude that the hypothesis has been definitely proved erroneous. However, on that very hypothesis such a run of heads is not excluded, since that hypothesis asserts something about the limiting ratio of heads in an *infinite* class and not in a *finite* one. In general, that hypothesis is compatible with *any* results obtained in any finite number of throws; and, conversely, a given result within a finite class of throws is compatible with any hypothesis about the numerical values of the probability. In short, it seems that no direct statistical evidence obtainable from actual trials (which must obviously be finite in number) can establish or refute a probability statement.

(It should be observed, moreover, that this difficulty is not obviated, as some writers have thought, by employing a less precise definition for 'probability.' For example, if we define it in terms of approximate ratios in long runs, a finite number of observations on the direct evidence for a probability statement will still not suffice to establish or refute it completely and unambiguously.)

The formal argument is as follows. If $f_1, f_2, \ldots, f_n, \ldots,$ is the series of relative frequencies of heads and ϵ a positive number, to say that the probability of getting a head is $\frac{1}{2}$ is to say that $\frac{1}{2}$ is the limit of these ratios. And this means that for every ϵ there is an N, such that for every n, if $n > N$, then the absolute difference of f_n and $\frac{1}{2}$ is less than ϵ. Or, in the notation of modern logic,

$$(\epsilon) \, (\exists \, N) \, (n) \, [(n > N) \supset (|f_n - \tfrac{1}{2}| < \epsilon)].$$

This statement contains three quantifiers, the two universal quantifiers 'for every ϵ' and 'for every n,' and the existential quantifier 'there is an N.' Because of the presence of the universal quantifiers, this statement cannot be established by examining a finite number of ϵ's and n's; or, in the language proposed by Carnap, the statement is not completely confirmable.

This situation is familiar throughout science. For example, the statement 'All bodies attract each other inversely as the square of their mutual distances' is not completely confirmable either. But, according to strict logic and textbook scientific method, this latter statement is capable of *complete disproof* by *one negative instance*; and it is usually said, therefore, that, although we

can never be in the position to assert the truth of universal statements, we may be in the position to assert their falsity.

However, probability statements do not fall under this dictum. For, in order to completely disprove such a statement, its formal contradictory would have to be completely confirmed. But the formal contradictory of the specimen probability statement is: There is an ϵ, such that for every N, there is an n, such that $n > N$, and the difference between f_n and $\frac{1}{2}$ is not less than ϵ. In symbolic notation

$$(\exists\, \epsilon)\, (N)\, (\exists\, n)\, [(n > N) \cdot (|f_n - \tfrac{1}{2}| \geq \epsilon)].$$

However, this statement also contains a universal quantifier, namely, 'for every N,' so that it cannot be completely confirmed.

In sum, therefore, a probability statement can be neither completely confirmed nor completely disconfirmed.

Many writers have therefore concluded that probability statements interpreted in terms of relative frequencies are devoid of empirical meaning because what they assert cannot be controlled by determinate empirical procedures. Such a conclusion, if it were warranted by the facts, would be fatal to a frequency interpretation of probability. For it is a cardinal requirement of modern science that its statements be subject to the criticism of empirical findings. This simply means that not every state of affairs can be confirmatory evidence for a given statement and that observable states of affairs must be specifiable which would be acknowledged as incompatible with its truth. On the other hand, such a conclusion is paradoxical because in actual practice probability statements interpreted in terms of frequencies *are* accepted or rejected on the basis of empirical evidence; and no one seriously doubts that we order affairs of everyday living, of industry, and of science with their help.

What is required, therefore, is a specification of the semantical and pragmatic *rules* in accordance with which probability statements are accepted and rejected on the basis of empirical findings. Although a complete set of rules cannot be given at present, so that the problem is in a very unsettled condition, it is believed that the following observations will be found relevant to the issue raised.

a) An objection often made to the limit definition of probability is that limits, in the strict sense of the term, do not exist for empirically determined relative frequencies and that in actual statistical material the ratios of frequencies fluctuate more or less widely. Such an objection, however, should in all consistency be made also to the use of general mathematical analysis in the natural sciences. For the limit concept is employed not only in probability but elsewhere also. For example, the masses or centers of gravity of bodies are frequently calculated with the help of the integral calculus, and the integrations are performed on the assumption that the mathematical functions which specify the density of the bodies are continuous; the calculation of these quantities thus involves limits at several places. Moreover, the assumption of a continuous density distribution is not warranted by our present theories of matter as discontinuous. We do not, however, reject the powerful tools of analysis for these reasons. An even simpler illustration of the use of limits occurs in measurement, against which no one seems to raise difficulties of the sort indicated. Every actual measurement, for example, of the length of the diagonal of a square, yields a *rational* number; nonetheless, in theoretical work we frequently employ irrational numbers, such as $\sqrt{2}$, for specifying lengths; and irrational numbers involve limit notions. The reason for employing terms involving limits in probability theory, as elsewhere, is the same: we thereby obtain powerful and economical methods in making *mathematical transformations.* And the reason why the use of such "calculus terms" and the procedures requiring them is countenanced in the natural sciences (even when direct empirical evidence and theoretical considerations indicate that the conditions for their use are not fully satisfied) is that we know how to *correlate* with them *groups* of directly measured magnitudes lying in certain intervals.

b) It is indeed a naïve conception of scientific method according to which the statements of science (whether singular or general) are to be rejected on the ground of a single negative instance. It was pointed out in Section I that, even in the exact

sciences of measurement, the numerical values of magnitudes as predicted by a theory are not in precise agreement with the numerical values obtained by actual measurement and observation; the theory of errors had its genesis in the study of just such situations. A theory is not in general dismissed as false or worthless because the confirmation of its predictions by observation is only approximate—even though *formally* every deviation from a predicted value of a magnitude is a negative instance for a theory. The amount of allowable deviation between predicted and observed values is not specified by the theory itself, and even a "large" deviation may not be decisive against the theory. The reasons for this are twofold: An empirically testable consequence of a theory does not follow from the theory *alone*, but from it conjoined with statements reporting matters of observation and possibly other theories. Consequently, an apparent negative instance for a theory may be argued to be incompatible not with *it* but only with some of the other premisses of the argument; and by a suitable alteration in the assumptions from which the testable consequences are drawn, the theory itself may be retained as in accordance with the "facts." Second, the amount of allowable deviation between predicted and observed values of a magnitude may be a function of a number of variable factors, such as the number of observations made, the purposes for which the inquiry is conducted, the kind of activity which the theory is intended to coordinate and foretell, or the character of the instruments by means of which the testing is carried on. These factors cannot in general be completely enumerated or specified in detail, although those who conduct researches have been trained to make allowance for them in the concrete cases before them.

A crude illustration of this second point, for the case when *direct statistical evidence* for a probability statement is evaluated, can be constructed as follows. Suppose the hypothesis that $\frac{1}{2}$ is the probability of obtaining a head with a coin is to be tested, by tossing it one hundred times. According to the hypothesis, we may expect approximately fifty heads. If heads turned up forty-nine times, we would regard this as confirming

the hypothesis; if heads turned up forty-five times, this may still be regarded as confirmatory; but if heads turned up only twenty times, we might suspect that the coin is loaded and doubtless propose a different value for the probability of getting a head. That is to say, somewhere between getting twenty and getting fifty heads in one hundred throws, we might fix a value such that a frequency less than it is to be taken as disconfirming the hypothesis of $\frac{1}{2}$. In other words, the actual hypothesis which would be tested under these circumstances is that the relative frequency of heads lies in an *interval* $\frac{1}{2} \pm \delta$, where the positive number δ is not fixed once for all but varies with circumstances. Now the probability of obtaining deviations of specified magnitudes from $\frac{1}{2}$ (on the assumption that *sets* of such trials are repeated indefinitely) can be calculated with the help of Bernoulli's theorem; and *this* probability depends upon the initial hypothesis that p is the probability of getting a head as well as upon the number n of throws which are made. Hence δ will often be a function of p and n. But it may be a function of other factors as well: e.g., of our knowledge of the physical construction of the coin and of the circumstances under which it is thrown, of the size of our fortune if we are gambling, etc. The definition of 'probability' in terms of 'limit' is therefore important for the purpose of constructing a consistent and powerful *calculus*. The calculus itself is instrumental in effecting transitions from one set of empirically controllable statements to other such sets. Provided that appropriate semantical and pragmatic rules are instituted for applying the calculus, it is not a serious objection to it that some of its terms cannot be taken as *descriptive* of the subject matter of science.

In modern theoretical statistics various methods have been devised for evaluating the goodness of an estimate of parameters, such as p, which characterize a hypothetical infinite population. According to the older methods of Lexis, the *aggregate sample* on the basis of which the estimate is made requires to be analyzed into sets of elements which are similar in certain relevant respects; the stability or fluctuation of the estimate in these various groups is then studied. In the more recent methods of R. A. Fisher, J. Neyman, and others, "measures of credibility" are introduced, some of which are

carefully distinguished from probabilities. According to these methods, the values assigned to the hypothetical probabilities must meet explicitly stipulated conditions of stability under repeated samplings, and must also make these measures of credibility a maximum. It is not possible at this place to enter into this subject in greater detail.

In many cases, no determinate numerical value can be assigned to a probability, not because a frequency interpretation of probability statements is not relevant but because relevant statistical information is lacking. For example, the proposition is often asserted that when the barometer falls it is highly probable that it will rain, although no numerical value is usually specified for this "high probability." Such a statement clearly means that the relative frequency of rain within a few hours, in the class of cases where the barometer falls, is greater than $\frac{1}{2}$ and possibly close to 1. But lacking precise statistical information, the high probability is assigned and confirmed on the basis of general impressions as to the behavior of the weather. In still other cases, such as that involved in estimating the probability of a witness speaking the truth, the statistical data may be even more meager, and the general impressions upon which we base our estimates may be highly unreliable and even worthless.

We have no final assurance that a hypothesis as to the numerical value of a probability is a correct one. However, the method of inquiry we employ is a self-corrective one, and in general we place greater reliance upon our rules of procedure and their *net* results than upon particular conclusions obtained. We are not in a position to assert with finality that the empirical frequencies we obtain do converge to a limiting value. But as Peirce and more recently Reichenbach have pointed out, if these ratios do tend to remain within certain narrow intervals, we can discover what those intervals are by a repeated and systematic correction of the estimates which are suggested by the samples we continue to draw.

c) Thus far, only the *direct statistical* evidence for a probability statement has been considered. But it was explained in Section II that, whenever such a statement is part of an in-

clusive system of statements, the evidence may be indirect and even of a nonstatistical character. There are, indeed, the following possibilities: Let S be a probability statement of the form "$\text{prob}(A, R) = p$"; and let Σ be a class of statements which in general will contain *singular* statements reporting matters of observation (e.g., statements which ascribe a property to a definite space-time region), as well as general or theoretical statements some of which may have the form of probability statements.

(i) The value of p in S may be estimated directly from statistical evidence concerning the frequency of A in R; this case has already been considered.

(ii) From S and Σ another probability statement S_1 may be derived which may be tested by direct statistical evidence for S_1. Thus, if S ascribes the probability of $\frac{1}{2}$ to a coin falling head uppermost, S_1 may ascribe the probability of $\frac{1}{4}$ to the coin falling heads up twice in succession.

(iii) From S and Σ a statement S_2 may be derived which is nonstatistical. Thus, let S ascribe the probability of $\frac{1}{2}$ to an atom in a state with a magnetic moment of one suffering a transition into a state with a magnetic moment of two when a deflecting field is introduced; then S_2 may assert that the intensity of the ionic current across the path of the molecular beam is of a specified magnitude. In this case, no problems arise in connection with the empirical control of probability statements which do not arise in connection with other statements of science.

(iv) The value of p in S may be deduced from Σ. Thus, if Σ contains the Schrödinger equation together with a number of boundary conditions, we can calculate the numerical value of the probability that an atom in a given space-time region will be a in a specified state.

Although some of the formal logical problems in connection with cases (iii) and (iv) have not been thoroughly worked out, such cases do occur. And it is evident from them that the correctness of a given hypothesis as to the numerical value of a probability may be controlled in much the same way as the

more familiar nonstatistical hypotheses of science are controlled.

In recent years, following a suggestion of Poincaré, what is sometimes known as "a causal theory of probability" has been developed by G. D. Birkhoff, E. Hopf, and others. The main idea of these researches is the deduction of a probability value (e.g., the probability of the ball in roulette coming to rest in a red sector) from underlying dynamical assumptions governing the average values of certain quantities with increase of time. It is incorrect to maintain, as some have done, that a probability value can be deduced from a dynamical theory which contains no material assumptions about the distribution of frequencies or average values. Nevertheless, these researches, apart from their technical interest, emphasize one very important point: An estimate of a probability which is made simply on the basis of unanalyzed samples or trials is not likely to be a safe basis for prediction. If nothing is known concerning the mechanism of a situation under investigation, the relative frequencies obtained from samples may be poor guides to the character of the indefinitely large population from which they are drawn. Thus, because we know very little about the mechanism of historical changes in human societies, it would be unsafe to use the life-probabilities computed in the first quarter of the present century as a basis for conducting a life insurance business in America two centuries hence. On the other hand, because we know something about the mechanism of biological heredity, a relatively few observations on the number and types of descendants of a plant may suffice to confirm hypotheses about the probability of certain types recurring. Again, we assign a value to the probability of getting heads on a freshly minted coin with great assurance, even before making any actual trials with it, because the homogeneity of the products of national mints, as well as of the conditions under which the coin would be thrown, are fairly well established. In general, therefore, the amount and kind of evidence required for probability statements depend on their interconnections with the body of our knowledge and theories at a given time.[13]

(v) Some writers, notably Reichenbach, have maintained that, while probability statements are incapable of complete confirmation or disconfirmation, nevertheless a degree of probability (in the frequency sense) can be attached to them. Such a proposal, it turns out, involves a hierarchy of probabilities, in which every probability statement on one level is subject matter for probability statements on a higher level; it is a conception which has stimulated the development of a "probability logic" referred to in Section II. If such a proposal could be implemented with an unambiguous and convenient method for assigning probabilities to probability statements, it would go a long way to solving definitively the logical problem to which the present section has been devoted. Reichenbach's writings make important contributions toward formulating such a method. However, a probability statement is a *general* statement, as was explained on page 52; and we reserve the discussion of the probability of general statements, hypotheses, or theories for § 8.

8. Probability and Degree of Confirmation or Weight of Evidence

At the outset of Section II two classes of statements contain ing the term 'probable' were distinguished. The members of the first class have now been shown to require a frequency interpretation, and the statements in it are subject to the rules of the calculus of probability. We shall now inquire whether the second class is similar to the first in these respects.

A common objection to the frequency theory of probability is that, although probability statements concerning single occasions or single propositions are often asserted and debated, it is meaningless to assert such statements in terms of the frequency theory. For example, writers like Keynes have urged that such statements as 'It is probable on the evidence that Caesar visited Britain' and 'The evidence makes it improbable that all crows are black' cannot be analyzed in terms of relative frequencies; and they have concluded that a conception of probability is involved in them which is different from, and "wider" than, the frequency view. Frequentists have retorted, quite

rightly, that such statements *are* without meaning, if they *literally* attribute a probability in the frequency sense to a single proposition; but frequentists have also urged that such statements do have significance if they are understood as *elliptic* formulations.

There is little doubt that many probability statements which are apparently about single propositions are incomplete formulations and that, when they are suitably expanded, they conform to the conditions required by the frequency theory. On the question, however, whether *all* probability statements about single propositions are to be analyzed in this way there is considerable difference of opinion. This disagreement not only divides frequentists from nonfrequentists like Keynes but it also represents a division among those who subscribe to a frequency interpretation for the first class of statements previously mentioned.

This difference of opinion concerning the range of applicability of the calculus of probability has a long history. Earlier writers on the subject believed that the calculus was the long-sought-for instrument for solving all problems connected with estimating the adequacy of evidence. In particular, it was maintained that the problems associated with establishing general laws on the basis of examined instances and with obtaining some measure for the reliability of predictions (the traditional problems of induction) were part of the subject matter of the mathematical theory of probability. Bayes's theorem and the Rule of Succession were commonly employed for these purposes, and Jevons explicitly regarded induction as a problem in inverse probabilities. On the other hand, writers like Cournot and Venn, two of the earliest writers to propose a frequency interpretation of the probability calculus, were equally convinced, though for different reasons, that the calculus was not relevant to the problems of induction. More recently, Keynes and Reichenbach, arguing from diametrically opposite standpoints, agree on the point that the term 'probable' can be given a consistently univocal meaning; and Reichenbach has given the most complete account at present available of how to extend the fre-

quency view to the consideration of the probability of scientific theories. But other contemporary frequentists, such as Carnap, von Mises, Neurath, and Popper, though supporting the frequency interpretation for a very large class of probability statements, do not believe such an interpretation is appropriate for every statement which contains the word 'probable.' This latter group of writers rejects the notion of a "logical probability" as developed by Keynes and others; but it distinguishes between 'probable' employed in the sense of 'relative frequency' and 'probable' employed in the sense of 'degree of confirmation' or 'weight of evidence.'

It is possible, therefore, to distinguish writers on probability according to the following schema: (1) Writers who interpret 'probable' in a *univocal* sense; such writers differ among themselves according as they accept the classical view, the view of probability as a unique logical relation, or the frequency view. (2) Writers who do not believe that the term 'probable' can be interpreted in precisely the same manner in every one of the contexts in which it occurs.

The present state of research, therefore, leaves the issue unsettled as to the scope of the frequency theory of probability. We shall examine the points at issue, but our conclusion will of necessity have to be highly tentative. We shall concern ourselves explicitly with statements ascribing a probability to a theory, because of lack of space; but the discussion will apply without essential qualifications to probability statements about singular statements like 'Caesar visited Britain,' whenever such probability statements are not analyzable as elliptic formulations involving relative frequencies. By 'theory' will be understood any statement of whatever degree of complexity which contains one or more universal quantifiers, or a set of such statements.

1. *The probability of theories.*—We begin with examining the proposal to interpret probability statements about theories in terms of relative frequencies; and, since Reichenbach has expounded this proposal more fully than anyone else, we shall examine his views. Reichenbach has given two distinct but

allied methods for defining "the probability of a theory." The first of these methods has received an improved formulation by C. G. Hempel, which avoids serious difficulties present in Reichenbach's own version. It should be noted that the definitions given by both methods are semantic ones.

a) Let T be some theory, for example, the Newtonian theory of gravitation. Let C_n be a class of n singular statements, each of which specifies an initial state of a system. (For from T alone, without the specification of initial conditions, no empirically controllable consequences can be obtained; thus, the mass, initial position, and velocity of a planet must be assigned before a future state of the planet can be predicted.) From every such statement with the help of T, other statements may be derived, some of which are empirically controllable by an appropriate correspondence. Therefore, let E_n be the class of n such singular statements derived from C_n with the help of T. We suppose that a one-to-one correspondence is established between the elements of C_n and E_n; and without loss of generality we shall suppose that every statement in C_n is true. (From a single statement in C an indefinite number of consequences belonging to E may be derived; but we can simply *repeat* a statement in C for every one of the distinct consequences drawn from it.) Let $\mathrm{nu}(E_n)$ be the number of statements in E_n which are true. The relative frequency with which a statement in E_n is true when its corresponding statement in C_n is true is given by $\mathrm{nu}(E_n)/n$. Suppose now that n increases indefinitely, so that C_n will include all possible true initial conditions for T, while E_n will include all the possible predictions which are made from them with the help of T. The numerical expression

$$\mathrm{prob}(E,\ C) = \lim_{n \to \infty} \frac{\mathrm{nu}(E_n)}{n}$$

will then be the probability that the consequences, obtained with the help of T from appropriate initial conditions, are true. This, in essence, is Reichenbach's first method of assigning a probability to a theory T.

Although the foregoing exposition requires supplementation

in several ways, there seems to be little question that a precise definition for 'the probability of a theory' can be given on a relative frequency basis. It is, however, by no means evident that such a definition formulates the concept people seem to be employing when they discuss the probability of theories.

(i) On the foregoing definition the probability of a theory is the limiting value of relative frequencies in an infinite ordered class E. This value is therefore independent of the *absolute number* of true instances in E, and is also independent of the absolute or relative number of instances in E which we know to be true *at a given time*. However, we often do say that on the basis of *definite evidence* a theory has some "degree of probability." Thus, a familiar use of this phrase permits us to say that, because of the accumulated evidence obtained since 1900, the quantum theory of energy is more probable today than it was thirty years ago. The foregoing definition is not suitable for this use of the phrase.

(ii) Because the probability of a theory is defined as the limit of relative frequencies, the probability of a theory may be 1, although the class E of its empirically confirmable consequences contains an infinite number of statements which are in fact false. This conclusion could follow even if some of these exceptions to the theory are ruled out as not being genuine negative instances (see the discussion of this point in § 7). But, according to the familiar usage of 'probability of a theory' already referred to, if a theory did have an infinite number of exceptions, not only would not a "high degree of probability" be assigned to it: it would be simply rejected.

(iii) It is difficult to know how even the approximate value of the probability of a theory, in Reichenbach's first sense, is to be determined. The situation here is not quite the same as for the probability statements which occur *within* a natural science and which have been already discussed in § 7. In the present case it does not seem possible to obtain other than direct statistical evidence for an assigned numerical value; for it is not apparent how a statement about the probability of theories can be part of an inclusive system, so that the statement might

possibly be confirmed indirectly, perhaps even by nonstatistical evidence. Reichenbach's proposal of a hierarchy of probabilities, according to which the probability of a probability statement may be estimated, postpones this problem by referring it to a higher level of probabilities; but postponing a problem does not solve it.

b) The second method proposed by Reichenbach for assigning a probability to a theory in a frequency sense depends upon the first method. The theory T under consideration will now be regarded as an element in an infinite class K of theories. These theories are supposed to be alike in some respects and unlike in others; and the theory T will share with a number of others in K a certain definite property P. (The following crude illustration may help fix our ideas: Suppose T is the Newtonian theory, and K the class of possible theories dealing with the physical behavior of macroscopic bodies. P may then be the property that the force functions in the theory are functions of the coordinates alone.) The probability of the theory T is then defined as the limit of the relative frequency with which theories in K, possessing the property P, have a probability in Reichenbach's first sense which is not less than a specified number q.

We can comment only briefly on this proposal.

(i) Although it is easy to introduce the reference class K and the property P in the formal definition, in practice it is by no means easy to specify them. The class K must not be selected too widely or arbitrarily, but no way is known for unambiguously grouping together a set of allegedly "relevant" theories. The difficulty is even greater in specifying the property P for a concrete case. We might wish to say, for example, that the theory of relativity is more probable than the Newtonian theory. But just what is the property P in this case on the basis of which they are to be distinguished?

(ii) We do not at present possess a sufficiently extensive collection of theories, so that appropriate statistical inquiries cannot be made with respect to them in accordance with this proposal. This proposal therefore completely lacks practical relevance. Indeed, there is some ground for suspicion that the pro-

posal would be feasible only if, as Peirce suggested, "universes were as plentiful as blackberries"; only in such a case could we determine the relative frequency with which these different universes exhibit the traits formulated by a theory under consideration.

(iii) If we could assign a probability value to a theory according to the first of Reichenbach's two proposals, there would be little need for estimating its probability by the second method. It is consistent with these proposals that a theory which has a probability of 1 on the first method, has the probability of only 0 on the second method. But since we are, by hypothesis, interested in that *one* theory, of what particular significance is it to know that theories of such a type have almost all their instances in conformity with the facts with only a vanishingly small relative frequency? This second proposal, like the first, does not therefore formulate the sense of those statements which assign a "degree of probability" to a theory on the basis of *given* finite evidence. For this second proposal does not permit us to talk *literally* about the degree of probability which *one definite theory* has on the evidence at hand; and it is just this which is intended when the evidence for a theory at one time is compared with the evidence at another time.

2. *Degree of confirmation or weight of evidence.*—These difficulties with the two proposals for assigning a probability to a theory, in the relative frequency sense of the term, are serious enough to have led competent students to seek a different interpretation for such statements. Guided by the actual procedure of the sciences, a long line of writers have urged that a different concept is involved in such statements than the one specified by the frequency theory of probability. This concept has been designated as "degree of confirmation" or "weight of evidence," in order to distinguish it from the various interpretations given to the term 'probable.' We shall briefly explain what is meant by 'degree of confirmation' and discuss some of the problems which center around its use.

The initial task which must be performed before a satisfactory account of 'degree of confirmation' can be given is a careful

analysis of the logical structure of a theory in order to make precise the conditions under which a theory may be confirmed by suitable experiments. This has been partially done by Carnap with considerable detail and refinement. We shall, however, not reproduce the results of his analyses, and shall employ distinctions inexactly formulated but which are familiar in the literature of scientific method. In particular, we shall take for granted the following, of which use has already been made: No theory (or for that matter no singular statement) can be established completely and finally by any finite class of observations. But a theory can be tested by examining its instances, that is, the singular sentences E derived with the help of the theory from the sentences C stating the initial conditions for the application of the theory. Both C and E may increase in number; but, while theoretically there are an infinite number of instances of a theory, no more than a finite number will have been tested at any given time. Indeed, a theory is said to be capable of being confirmed or verified only incompletely, just because no more than a finite number of its instances can be actually tested. The instances may be confirmed by observation, in which case they are called the *positive instances* for the theory; or they may be in disaccord with the outcome of observations, in which case they are called the *negative instances*.

We shall assume for the sake of simplicity that there are no negative instances for a given theory T. Then as we continue the process of testing T, the number of positive instances will usually increase. Now it is generally admitted that, by increasing the positive instances, the theory becomes more securely established. What is known as 'the weight of evidence' for the theory is thus taken to be a function of the number of positive instances. And we may accordingly state as a preliminary explanation of what is meant by 'the degree of confirmation' for a theory that the degree of confirmation increases with the number of the positive instances for T.[14]

This explanation is, of course, far from precise; but at present no precise definition for the term is available. As matters stand, the term is used in a more or less intuitive fashion in the actual

procedures of testing theories. It would obviously be highly desirable to have carefully formulated semantical rules for employing the term; but there is no early prospect that the rules for weighing the evidence for a theory will be reduced to a formal schema. The following observations, however, indicate some of the conditions under which the weighing of evidence is carried on, and will contribute something to making more precise the meaning of 'degree of confirmation.'

a) It does not seem possible to assign a quantitative value to the degree of confirmation of a theory. Thus, at one stage of investigation a theory T may have twenty positive instances in its favor, while at a later stage it may have forty such instances. While the degree of confirmation of T at the second stage would in general be acknowledged as greater than at the first stage, it is nevertheless not appropriate to say one degree of confirmation is twice the other. The reason for this inappropriateness is that, if degrees of confirmation could be quantized, all degrees of confirmation would be comparable and be capable therefore of a linear ordering. That this does not seem to be the case is suggested by the following hypothetical situation.

Suppose that the positive instances for T can be analyzed into two nonoverlapping classes K_1 and K_2, such that the instances in K_1 come from one field of inquiry and those in K_2 from another field. For example, if T is the Newtonian theory, K_1 may be the confirmatory instances for it from the study of planetary motions, while K_2 may be those coming from the study of capillarity phenomena; each set of instances is in an obvious sense qualitatively dissimilar from the other. Now imagine the following possibilities as to the number of instances in K_1 and K_2:

	P_1	P_2	P_3	P_4	P_5	P_6	P_7	P_8	P_9
K_1......	50	50	100	101	99	100	200	100	198
K_2......	0	50	0	49	52	90	0	100	2
E......	50	100	100	150	151	190	200	200	200

The last row of figures gives the total number of positive instances for T. These nine possibilities are arranged in order of

increasing number of positive instances. Would we say, however, that this order also represents the order of increasing degrees of confirmation?

It would generally be granted that for both P_2 and P_3 the degree of confirmation is greater than for P_1, simply because of the total number of positive instances. On the other hand, many scientists would be inclined to assign a greater degree of confirmation to P_2 than to P_3, even though the total number of positive instances is the same in these cases. And the reason they would give is that in P_2 there are *different kinds* of instances, while in P_3 there is only one kind. For this reason also P_6 would be assigned a higher degree of confirmation than P_7, even though the total number of positive instances in the former case is less than in the latter case. Again, P_4 and P_5 would often be assigned the *same* degree of confirmation, even though the total number of instances is different in these cases, because the relative number of instances of each kind is approximately the same. Finally, P_8 and P_9 would often be regarded as *incomparable* with respect to their degrees of confirmation, because of the disparity in the relative number of different kinds of instances.

Variety in the kinds of positive instances for a theory is a generally acknowledged factor in estimating the weight of the evidence. The reason for this is that experiments which are conducted in qualitatively different domains make it easier to control features of the theory whose relevance in *any* of the domains may be in question. Hence, by increasing the possibility of eliminating what may be simply accidental successes of a theory under special or unanalyzed circumstances, the possibility of finding negative instances for the theory is increased. In this way of conducting experiments, the theory is subjected to a more searching examination than if all the positive instances were drawn from just one domain. A large increase in the number of positive instances of one kind may therefore count for less, in the judgment of skilled experimenters, than a small increase in the number of positive instances of another kind. It follows, however, that the degree of confirmation for

a theory seems to be a function not only of the absolute number of positive instances but also of the kinds of instances and of the relative number in each kind. It is not in general possible, therefore, to order degrees of confirmation in a linear order, because the evidence for theories may not be comparable in accordance with a simple linear schema; and a fortiori degrees of confirmation cannot, in general, be quantized.

Indeed, the foregoing hypothetical situation is only a highly simplified outline of the considerations which are usually taken to be relevant in estimating the weight of the evidence for a theory. Among other factors usually considered is the precision with which the confirmable consequences of a theory are in agreement with experimental findings. Although, as has been repeatedly explained, a theory is not rejected simply because perfect agreement between predicted and experimentally determined magnitudes does not occur, the more closely the observed values center around the theoretically expected magnitudes, the greater weight is usually attached to the supporting observations for a theory. Furthermore, evidence for a theory often consists not only of its own positive instances but also of the positive instances for *another* theory, related to the first within a more inclusive theoretical system. The number of direct positive instances may in such cases be regarded as of small importance, in comparison with the fact that support is given to the theory by the accumulated positive instances for the inclusive system.

b) How large must the number and kinds of positive instances be in order that a theory can be taken as adequately established? No general answer can be given to such a question, since the answer involves practical decisions on the part of those who conduct a scientific inquiry. There is an ineradicable conventional element among the factors which lead to the acceptance of a theory on the basis of actual evidence at hand. It is always theoretically possible to demand further evidence before agreement is reached that a theory has been sufficiently well tested. However, the practical decision is in part a function of the contemporary scientific situation. The estimation

of the evidence for one theory is usually conducted in terms of the bearing of that evidence upon alternative theories for the same subject matter. When there are several competing theories, a decision between them may be postponed indefinitely, if the evidence supports them all with approximately the same precision. Furthermore, the general line of research pursued at a given time may also determine how the decision for a theory will turn out. For example, at a time when a conception of discontinuous matter is the common background for physical research, a theory for a special domain of research formulated in accordance with the dominant leading idea may require little direct evidence for it; on the other hand, a theory based on a continuous notion of matter for that domain may receive little consideration even if direct empirical evidence supports it as well as, or even better than, it does the alternative theory.

In particular, the acceptance of definite numerical values for probabilities also involves practical decision, for which no general rules can be given. As already explained, such numerical values are often computed on the basis of more or less comprehensive theoretical systems, and the confidence we have in the correctness of those values depends on the confidence we have in those systems. It may happen that we can determine the value of a probability with only small accuracy by a theory which has a relatively high degree of confirmation, while a different value may be computed with great precision by an alternative theory with an inferior degree of confirmation. The supposition that in such a case the dilemma can be resolved by a clear-cut method neglects the human and accidental factors which determine the history of science. Certainly no mathematical or logical formula can be given which would mechanically supply a coefficient of weight for the correctness of the decisions which are made in many analogous cases.

c) Assuming that these desultory observations are based on the study of actual scientific procedure, it may be asked why it is that we seem to feel that theories with a greater degree of confirmation deserve our confidence on logical grounds more than those with less—whenever such comparisons can be made.

Why, in other words, should a theory be regarded as "better established" if we increase the number and kinds of its positive instances?

Perhaps a simple example will help suggest an answer. Suppose a cargo of coffee is to be examined for the quality of the beans. We cannot practically examine every coffee bean, and so we obtain some sample beans. We do not, however, sample the cargo by taking a very large number of beans from just one part of the hold; we take many relatively small samples from very many different parts of the ship. Why do we proceed in this way? The answer seems to be that our general experience is such that, when we conduct our samplings in this manner, we approximate to the distribution of qualities in the entire hold; and, in general, the larger our individual samples and the more diversified our choice of the parts of the ship from which they are taken, the more reliable (as judged by subsequent experience) are the estimates we form. It is at least a plausible view that in testing a theory we are making a series of samplings from the class of its possible instances. A theory is "better established" when we increase the number and kinds of its positive instances, because the *method* we thereby employ is one which our general experience confirms as leading to conclusions which are stable or which provide satisfactory solutions to the specific problems of inquiry. At any rate, this was the answer which Charles Peirce proposed to the so-called "problem of induction," and which has been independently advanced in various forms by many contemporary students of scientific method (e.g., M. R. Cohen, J. Dewey, H. Feigl, O. Neurath, and many others). As Peirce succinctly put the matter, "Synthetic inferences are founded upon the classification of facts, not according to their characters, but *according to the manner of obtaining them.* Its rule is that a number of facts obtained in a given way will in general more or less resemble other facts obtained in the same way; or, *experiences whose conditions are the same will have the same general characters.*" A degree of confirmation is thus a rough indication of the extent to which our general *method of procedure* has been put into operation. While no prob-

ability in a frequency sense can be significantly assigned to any formulation of our method (because it is that very method which is involved in estimating and testing such probabilities), scientific inquiry is based upon the assumption, which is supported by our general experience, that the method of science leads to a proportionately greater number of successful terminations of inquiry than any alternative method yet proposed.[15]

Attempts to find a systematic answer to "the problem of induction" within the framework of a theory of probability, though often made, have not in general been regarded as successful. The *process* of induction has been usually conceived as the search for more or less stable and pervasive relations between properties of objects; and the *problem* of induction has been taken to be the discovery of a principle (the principle of induction) which would "justify" the various conclusions of that process. Stated in this way, it is rather difficult to know just how the "problem" is to be conceived in empirical terms. On the face of it, the "problem" seems to involve a futile infinite regress; and indeed the Achilles heel of attempted solutions of it has usually been the status of the proposed principle of induction: how is the principle itself to be "justified"? The number of different types of answers which have been given to this last question is relatively small; among them are the following: the inductive principle is a synthetic a priori proposition concerning the nature of things in general, it is an a priori proposition concerning the fundamental constitution of the human mind, it is a generalization from experience, and it is a "presupposition" or "postulate" of scientific procedure. It would take too long to examine these answers in detail. It is perhaps sufficient to note that the first two involve positions incompatible with the conclusions of modern logical research; that the third commits a *petitio principii;* and that the fourth, assuming it to have a clear meaning, cannot make of the proposed inductive principle a "justification" of the procedure of science or of its conclusions, since according to this answer the principle is simply an *instrument* of scientific procedure. The position taken in the present monograph is that no antecedent principle is required to justify the procedure of science, that the sole justification of that procedure lies in the specific solutions it offers to the problems which set it into motion, and that a *general* problem of induction in its usual formulation does not exist. Since the notion of the probability of theories (in the specific senses discussed above) has been found to involve serious difficulties, and since the degree of confirmation for a theory has been argued to indicate the extent to which the theory has been tested by the procedure of science, the problem of induction which the present writer recognizes as genuine is the formulation of the general features of scientific method—of the method which, in short, leads to a proportionately greater number of successful terminations of inquiry than the number which other methods may have to their credit.

One brief final remark: It has been customary in the traditional discussions of scientific theories to seek grounds for our knowledge of their *truth* or at least of their *probability* (in some one of the many senses previously discussed). Omitting more than mention of those students (e.g., Wittgenstein and Schlick) who have dismissed such discussions as meaningless because, according to them, theories are not "genuine" propositions since they are not completely verifiable, reference must be made to another group of writers. According to this group, the traditional discussions have not fruitfully illuminated the character of scientific inquiry because those who take part in them neglect the *function* which theories have in inquiry. When this function is examined, it has been urged, it turns out that questions of the *truth* of theories (in the sense in which theories of truth have been traditionally discussed) are of little concern to those who actually use theories. Reflective inquiry is instituted for the sake of settling a *specific* problem, whether it be practical or theoretical, and inquiry terminates when a resolution of the problem is obtained. The various procedures distinguishable in inquiry (such as observation, operation upon subject matter including the manipulation of instruments, symbolic representation of properties of subject matter, symbolic transformation and calculation, etc.) are to be viewed as instrumental to its end product. The use of theories is one patent factor in reflective inquiry. They function primarily as means for effecting transitions from one set of statements to other sets, with the intent of controlling natural changes and of supplying predictions capable of being checked through manipulating directly experienceable subject matter. Accordingly, in their actual use in science, theories serve as *instruments* in specific contexts, and in this capacity are to be characterized as good or bad. effective or ineffective, rather than as true or false or probable. Those who stress the instrumental function of theories are not necessarily committed to identifying truth with effectiveness and falsity with uselessness. Their major insight does not consist in denying the meaningfulness of certain types of inquiries into the truth of theories but in calling attention to the way theories

function and to the safeguards and conditions of their effectiveness. A theory is confirmed to the degree that it performs its specific instrumental function. From this point of view, which has been developed with much detail by Dewey, the degree of confirmation for a theory may be interpreted as a mark of its proved effectiveness as an intellectual tool for the purposes for which it has been instituted.

9. Concluding Remarks

In consonance with the discussion and terminology of the theory of signs in Volume I, Number 2, it is convenient to classify the problems connected with probability into three distinct though connected groups. *Syntactical problems:* these are concerned primarily with perfecting the calculus of probability, making more precise its assumptions, simplifying its procedure, establishing its consistency, developing alternative formal techniques, and indicating its relation to other branches of formal mathematics. Some of these matters were considered in Section II. *Semantical problems:* these are concerned with establishing and formulating appropriate rules for applying the calculus to various existential affairs, by indicating under what conditions certain complexes of signs in the calculus are to be co-ordinated with experimentally controllable situations. *Pragmatic problems:* these are concerned with formulating the procedures and conditions involved in the acceptance of probability statements, and with evaluating the efficacy of the calculus in solving the problems set for it in scientific inquiry. Semantical and pragmatic problems were outlined in the present section.

In recent years a growing number of mathematicians and logicians have devoted themselves to the solution of the syntactical problems of probability. Although there are still a number of outstanding difficulties, these are being attacked with the most subtle instruments of modern mathematics. In any case, the calculus has been refined and generalized to an extent undreamed of a century ago. The discussion of the semantical problems of probability is perhaps still in its infancy, though important spade work has already been done. The very

recognition of the existence of such problems bodes well for the future, since the classical discussions of probability have been shown to be inadequate largely because semantical problems were not clearly distinguished from syntactical ones. The discussion of pragmatic problems has been carried on in the United States for many years. The most obvious fruits of this activity are the number of substantial contributions to an objective psychology dealing with scientific inquiry. And the present cooperative attack upon this group of problems by biologically oriented thinkers and those trained in the mathematical sciences gives a bright promise that, perhaps for the first time in the modern period, an adequate account of human behavior in the context of getting knowledge will soon be available.

The present section has stressed problems associated with the discussion of probability which are still largely unsettled. An unsettled situation in an intellectual discipline has often been seized upon by those hostile to free inquiry as an opportunity to cry out the "bankruptcy of science," to charge it with "confusion," to preach a wholesale skepticism with respect to its findings, and to invoke dogmatically "perennial truths" in the interest of private and institutionalized vested interests. However, unsettled situations in science usually mark important departures from traditional modes of analysis and are concomitants of active research; and the present state of probability discussions is typical of such situations. Disagreement among competent students certainly indicates that the last word upon the topic under discussion has not been said; but it may also indicate that a community of workers is co-operatively engaged in contributing to the solution of complicated issues. Such in fact is patently the case in current discussions of probability. Even where sharp disagreements occur, those engaged in the discussion have been drawing upon one another's insights, have been influencing one another to state their proposed solutions with greater precision, have been led to recognize alternative possibilities in solutions, and have consequently guarded themselves against a premature commitment to theses which may block the course of further inquiry. What is essen-

tial for the future development of probability considerations, as for the development of science in general, is that trained minds play upon its problems freely and that those engaged in discussing them illustrate in their own procedure the characteristic temper of scientific inquiry—to claim no infallibility and to exempt no proposed solution of a problem from intense criticism. Such a policy has borne precious fruit in the past, and it is reasonable to expect that it will continue to do so. In the history of the study of probability it has brought into existence a perfected calculus of probability; it has led to an extension of its range of application to many diverse domains; and it has contributed to showing that the various sciences, however distinct their specific subject matters may be, employ a common logic and common procedures, are faced with common logical problems, and are mutually indebted to one another for indispensable tools of inquiry.

NOTES

These very limited bibliographical notes aim to do no more than suggest further reading on some of the classical topics mentioned in the text.

1. For the classical view of probability consult Laplace, *Essai philosophique sur les probabilités* (Paris, 1814); A. De Morgan, *An Essay on Probability* (London, 1838). For the logical view see J. M. Keynes, *Treatise on Probability* (London, 1921); J. von Kries, *Die Principien der Wahrscheinlichkeitsrechnung* (Tübingen, 1886); F. Waismann, "Analyse des Wahrscheinlichkeitsbegriffs," *Erkenntnis*, Vol. I. For the frequency view see J. Venn, *Logic of Chance* (London, 1886); Charles S. Peirce, *Collected Papers*, Vol. II (Cambridge, Mass., 1932); R. von Mises, *Wahrscheinlichkeit, Statistik, und Wahrheit* (Vienna, 1936) and *Wahrscheinlichkeitsrechnung* (Leipzig, 1931); H. Reichenbach, *Wahrscheinlichkeitslehre* (Leiden, 1935).

2. For these alternative definitions consult K. Popper, *Logik der Forschung* (Vienna, 1935), and A. H. Copeland, "Admissible Numbers in the Theory of Probability," *American Journal of Mathematics*, Vol. L, as well as his "Predictions and Probabilities," *Erkenntnis*, Vol. VI.

3. Peirce's comments on these matters are scattered throughout his writings, especially in Vols. II and VI of his *Collected Papers*. Philipp Frank has written many monographs on this subject, but the fullest account will be found in *Das Kausalgesetz und seine Grenzen* (Vienna, 1932). Henry Margenau develops his point of view in several articles in *Philosophy of Science* and in R. B. Lindsay and H. Margenau, *Foundations of Physics* (New York, 1936). See also E. Cassirer, *Determinismus und Indeterminismus in der moderne Physik* (Göteborg, 1937).

4. Reichenbach's book cited in n. 1 contains fairly full references to these discussions. A. Wald's paper, "Die Widerspruchsfreiheit des Kollektivbegriffes der Wahrscheinlichkeitsrechnung," appeared in K. Menger's *Ergebnisse eines mathematischen Kolloquiums*, Heft 8.

Principles of the Theory of Probability

5. The technical details referred to will be found in any book on the mathematical theory of probability; for example, the books of von Mises cited in n. 1. Fisher's writings are scattered in many periodicals but are summarized in his *Statistical Methods for Research Workers* (Edinburgh and London, 1925), which also contains a list of bis papers, and also in his *The Design of Experiments* (Edinburgh and London, 1935).

6. This criticism has been stated by a number of writers, e.g., F. Cantelli, "Considération sur la convergence dans le calcul des probabilities," *Annales de l'Institut Henri Poincaré*, Vol. V; T. C. Fry, *Probability and Its Engineering Uses* (New York, 1928); and R. B. Lindsay and H. Margenau in their *Foundations of Physics*.

7. Consult A. Kolmogoroff, "Grundbegriffe der Wahrscheinlicbkeitsrechnung," *Ergebnisse der Mathematik*, Vol. II, and the important series of works edited by E. Borel entitled *Traité du calcul des probabilités et de ses applications*.

8. These interpretations will be found in F. P. Ramsey, *Foundations of Mathematics* (London and New York, 1931); C. G. Hempel, "Über den Gehalt von Wahrscheinlicbkeitsaussagen," *Erkenntnis*, Vol. V; H. Jeffreys, *Scientific Inference* (Cambridge, 1931); S. Mazurkiewicz, "Über die Grundlagen der Wahrscheinlichkeitsrechnung," *Monatshefte für Mathem. u. Physik*, Vol. XLI; and other works by authors already cited in previous notes.

9. Reichenbach's views have been stated by bim in their most complete form in the work cited in n. 1 and in his *Experience and Prediction* (Chicago, 1938).

10. Consult G. Birkhoff and J. von Neumann, "The Logic of Quantum Mechanics," *Annals of Mathematics*, Vol. XXXVII; M. Strauss, "Zur Begruendung der statistiscben Transformationstheorie der Quantenmechanik," *Sitzungsber. der preuss. Akad. d. Wiss.* (1936); Paulette Février, "Les Relations d'incertitude de Heisenberg et la logique," *Comptes rendus des sciences*, Vol. CCIV.

11. For a recent exposition of the subjective view consult B. De Finetti, "La Prevision: ses lois logiques, ses sources subjectives," in *Annales de l'Institut Henri Poincaré*, Vol. VII. Criticisms of the classic view will be found in the writings of Peirce, Venn, von Kries, and Keynes already referred to.

12. In addition to the works cited in n. 1, consult J. Nicod, *Le Probleme logique de l'induction* (Paris, 1923).

13. Poincaré's method is explained in his *Calcul des probabilités* (Paris, 1896) and also by Reichenbach in his *Wahrscheinlichkeitslehre*. For the work of E. Hopf see "On Causality, Statistics and Probability," *Journal of Mathematics and Physics*, Vol. XIII; see also G. D. Birkhoff and D. C. Lewis, "Stability in Causal Systems," *Philosophy of Science*, Vol. II.

14. Carnap's discussion is contained in his "Testability and Meaning," *Philosophy of Science*, Vols. III and IV.

15. For further discussion of these matters consult M. R. Cohen, *Reason and Nature* (New York, 1931); John Dewey, *Essays in Experimental Logic* (Chicago, 1916), *Quest for Certainty* (New York, 1929), and *Logic: The Theory of Inquiry* (New York, 1938); H. Feigl, "The Logical Character of the Principle of Induction," *Philosophy of Science*, Vol. I; Otto Neuratb, "Pseudorationalismus der Falsifikation," *Erkenntnis*, Vol. V.

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Foundations of Physics

Philipp Frank

Foundations of Physics

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I. Introduction

In the present monograph an attempt is made to present physics in such a way that it is fit to become part of unified science. The first step is to bring into such a shape that it becomes clear which statements tell something about observable facts and which are statements about the choice of symbols. This means that we must discuss the operational meaning of all symbols used and the kind of relations which exists between these symbols.

We are not going to set up a complete system of symbols and operational definitions from which one could derive all facts of physics. Such a systematic presentation would be a very hard job, and at that I suspect that only a very few scientists would read it. Most of this presentation would be a dull routine work, and the reader would not know how to find the points to which he should direct his attention. The scientist is interested in logical analysis if, and only if, this analysis is not trivial or commonplace. One does not need to occupy each square foot of a territory in order to control it. What matters is only the control of some key positions. In this presentation of physics we shall attempt to bring under control only a few key positions in the huge symbolic structure by which the scientists have been "mapping" the wide domain of physical phenomena.

It is not difficult to find out these key positions. We must look only for the starting-points of current "interpretations" of physics on behalf of some philosophical creed, usually idealistic, or even spiritualistic. If we examine these "interpretations", we notice almost regularly that they are not interpretations of physics itself but interpretations of the symbolic structure, ignoring the operational meaning of these symbols. This means that they ignore the link between symbols and observable phenomena. Examples of this type of interpretation are obvious. The "energetic" aspect of physics is interpreted as a "refutation of materialism" and the four-dimensional presentation of relativity theory as establishing the "existence" of a four-dimensional world.

It would be a great mistake to say that the philosophers are chiefly responsible for these misinterpretations. As a matter of fact, each one of these "interpretations" has had its origin in a confused presentation of physics given by a physicist. Some of the latter have been honestly surprised at the fruits of their work. They have blamed the confused way of presenting their subject on the present "crisis" of physics, which leaves this field in a state of transition. One physicist suggested even putting up a sign telling all nonphysicists and in particular

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philosophers: "Keep out, while repairs are going on!" This attitude of "isolationism" would frustrate all attempts toward the unification of science and is, therefore, quite opposed to the aims of this *Encyclopedia* in general and of this monograph in particular.

Physics has been for centuries the spearhead of advance in human thought. In a unified science it should keep its role as a description of the physical universe and should not deteriorate into an incoherent, and somehow mysterious, agglomeration of symbols, rules, and recipes. In this situation quite a few physicists have tried to avoid all these difficulties by "sticking strictly to the facts" and by keeping away from the dangerous enterprise of logical and critical analysis. There is no doubt that this attempt is doomed to failure. Scientists who looked at the world from such different angles as Ernst Mach and A. N. Whitehead have agreed on one point: a physicist who dodges all logical analysis and tries to be a "physicist and only a physicist" will imbue the presentation of his subject with some "chance philosophy", usually a very obsolete one.[1] A careful examination of the presentation of physics given by physicists who pretend to keep strictly to the "facts and only to the facts" will reveal soon that these presentations are written in a spirit of medieval scholasticism. We can notice it even by a perfunctory look at textbooks for beginners. Quite a few make a generous use of words, like "entity", which

are borrowed from medieval philosophy and have no status in science.

Obsolete philosophical views in physics are mostly obsolete physical theories in a state of petrification.[2] Aristotle's philosophy of physics is a petrification of a physical theory which covered the experiences of Greek and oriental artisans about physical phenomena. Kant's *Metaphysical Principles of Natural Science* is a petrification of Newton's physics. Eddington's *Philosophy of Physical Science* is a petrification of Einstein's relativity and Bohr's quantum theory.

Remembering these facts, we can easily understand why a certain type of confusion in the presentation of physics gives rise to philosophical "interpretations". The lack of coherence in the presentation of a new physical theory often has its origin in the incoherence between the languages of old and new physical theories. The old language slips easily into the presentations of recent physics. For it pretends to express a certain philosophy which is based upon intuition or good sense and cannot be disregarded by physicists. Actually this language has been invented in order to present older physical theories. Its use means, therefore, confusing the language of recent physics with the language of older and abandoned theories. We can now understand why the key points in the structure of physics are the points where philosophical misinterpretations slip in easily. A coherent presentation of these key points and their vicinity will lead easily to a coherent

presentation of the whole domain of physics.

The elucidation of the relations between symbols and facts in physics owes much to the works of men like Ernst Mach, Henri Poincaré, C. S. Peirce, and P. W. Bridgman. The schools of thought which have been called Positivism, Pragmatism, and Operationalism have done a fine job. The present monograph follows their lines in general but does not make any attempt to build up three "self-consistent" systems of Positivism, Pragmatism, and Operationalism. The spirit of all these schools of thought is one and the same. But the theses which we would obtain if we tried to make a clear-cut distinction between them would result in three new branches of metaphysics: positivistic, pragmatistic, and operationalistic metaphysics.[3]

II. The Logical Structure of Physical Theories

1. Equations, Logical and Semantical Rules

Every physical theory consists of three essential parts.[4] There are, first, the equations of the theory, e.g., in mechanics, Newton's equation of motion; in electromagnetism, Maxwell's equations of the electromagnetic field; etc. These equations contain terms like 'co-ordinate', 'time', 'force', 'magnetic field intensity', 'electric conductibility', etc. By themselves they cannot be checked to see whether they are in agreement with the physical facts or not. Nor can we check their logical consistency. Carnap calls these equations the "calculus" of the field of physics.[5] Newton's equations of motion are, e.g., the calculus of mechanics. These equations must have as their basis, as Carnap expresses it, a second calculus by which we can learn what transformation of our equations are allowed without altering their meaning. Mechanics needs as a basis the calculus of algebra and geometry. Only by the application of this "second" calculus can the consistency of the first one be checked.

However, the system of both equations and logical rules (e.g., Newton's equations of motion plus the laws of algebra and geometry) is by no means a system of physical laws. We have to add, as a third part, statements which define the physical meaning of words like 'distance', 'time', 'moment', 'simultaneously', 'force', 'mass', etc. The statements which give these definitions are called by Carnap "semantical rules".[6] Besides the terms of the first calculus ('force', 'mass', etc.) and the terms of the second part, the logical rules ('plus'/'minus', 'and'/'or', etc.), the third part, the semantical rules, contains words like 'iron cube', 'wooden bar', 'warm water', 'one inch long', etc.

These latter words are to be understood in the sense in which they are used in our everyday language. With-

out this loan taken from the language of our daily life, the language of science would not be shareable knowledge. The semantical rules connect our equations or our calculus with the words of the language of our daily life, at least in physics and, perhaps, elsewhere too. But, in order to avoid any ambiguity, we have to be sure never to apply these words in a wider domain than in the range of their applications in our daily life. In this case this language is understood without controversy by everybody. But it is, e.g., illegitimate to apply the expression 'iron cube' to a body of a size of one million miles through, for we do not even know whether the existence of an iron cube of these dimensions would not violate the laws of physics. The same precaution has to be taken in using words like 'distance' or 'simultaneous'. As Carnap points out, only by adding these semantical rules to the equations do the latter become physical laws which can be checked by experiments.[7]

2. Operational Meaning and Validity of a Theory

In physics these semantical rules consist in the description of physical operations. Bridgman has studied the way the rules have been set up and used by the physicists long before the term 'semantics' had been used in logic.[8] To emphasize their character in physics, Bridgman calls these rules "operational definitions". When we know these rules or definitions, we

know "the operational meaning" of a term. We must, therefore, always be aware of the fact that, e.g., Newton's equations of motions by themselves do not contain the theory of motion. Newton's equations with their operational meaning provide us with the theory of motion. There is no way of checking whether the equations of motion by themselves are valid or true or correct. They are only a part of the theory. We can check only whether a system of equations plus the operational definitions is confirmed by a certain experiment or not.

If there is no disagreement with the experiment, we can say only that the system as a whole is "confirmed". But we cannot say that the equations by themselves or the operational definitions by themselves are confirmed. It is possible to alter the equations and the operational definitions in such a way that some conclusions drawn from the whole system remain unaltered.

Speaking exactly, we have to remember that experiments confirm a system which consists not only of two but of three kinds of statements: equations, plus operational definitions, plus logical and mathematical rules. Therefore, it is theoretically also possible to alter the traditional rules of logic without altering the confirmed conclusions.[9] We have only to introduce suitable alterations of the equations and the operational definitions. By an experimental confirmation we cannot demonstrate that the new logical rules are better than the old ones.

We demonstrate only that this new logic is a part of a system of statements which is as a whole in agreement with the experimental facts.

These remarks have their basis in a very simple argument of elementary logic which Bertrand Russell once elucidated by a drastic example: It is possible to add to these equations in a from the assumptions that "bread is made of stone" and that "stone is nourishing". Then it follows logically that "bread is nourishing". This statement can be confirmed by experiments. If someone claims for this reason that we have confirmed our assumptions, he would certainly be ridiculous. According to Russell, a great many confirmations of physical hypotheses are of this type.[10]

Now what is the meaning of the assertion that Newton's equations or Maxwell's equations themselves are confirmed by experiments? This assertion means, speaking exactly: It is possible to add to these equations in a fairly simple way such operational definitions that the whole system is in agreement with the observed facts. This confirmation also reveals, to be sure, a property of the equations themselves. If, e.g., Newton's equations had not this property, it would be impossible to find handy operational definitions which, added to these equations, would turn them into confirmed physical statements. We would need operational definitions which are complicated and hard to handle. But if such an addition is possible in a fairly simple way, we would say that Newton's equations themselves are confirmed by experiments. If we speak a little loosely, we may say that in this sense the equations themselves are physical laws.

But, even so, the operational definitions of general terms like 'energy', 'entropy', etc., are of an immense complexity. Bridgman has described the difficulties involved in an elaborate way. According to his analysis, "paper and pencil operations" play a great part. He has pointed out that only under very special conditions is it possible to distinguish by reasonable physical operations even between expressions which have apparently as simple a meaning as 'heat conduction' and 'heat radiation' or 'flux of heat' and 'flux of mechanical energy'.[11]

3. Are the General Laws of Nature Pure Conventions?

There has been a school of thought which has assumed that eventually one could find for any set of equations a set of operational definitions which might be added to these equations and turn them into confirmed physical laws. In this case an experimental confirmation would not reveal any property of the equations at all. The equations by themselves would not say anything about the physical world. However, if operational definitions of some of the symbols in the equations were given, the equations would become operational definitions of the rest of the symbols. Newton's equations of motion, e.g., would no longer be laws of motion. If we added to them

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the operational definitions of 'acceleration' and 'mass', the equations would become operational definitions of 'force' or a convention how to use the term 'force'.

The school of thought which has ascribed this property to all the fundamental equations of physics has been given the name "conventionalism". The French mathematician Henri Poincaré has been quoted as its most prominent spokesman. Actually, Poincaré emphasized that an experimental confirmation of equations plus operational definitions does not confirm the equations and that, by admitting any imaginable operational definition, we can turn every equation into a confirmed one. This statement can, from the angle of formal logic, hardly be refuted. It is logically equivalent to the statements of section 2. The equations, by themselves, are said to be "valid" or confirmable by experiments only if, by substituting "simple and practical" operational definitions, they become confirmed physical laws. This does not exclude that, by admitting all imaginable operational definitions, almost any system of equations could be converted into confirmed laws, provided that the system is not self-contradictory. If we consistently make the distinction between "simple and practical" operational definitions and arbitrary definitions which may be "complicated and impractical", it becomes clear in what sense the general laws of physics are purely conventional and in what sense they are valid assertions about facts.[12]

We understand this distinction easily if we examine the way in which operational definitions are actually used in physics. A specific operational definition can practically be used only if some specific physical laws are assumed to be valid. What is, e.g., the operational definition of the time distance "one hour"? We may say that this is the time during which the big hand of our pocket watch traverses an angle of 360 degrees. However, we are not interested in the reading of a particular watch. We mean to say that any pocket watch can be used. This means assuming that the hands of all pocket watches proceed with one and the same angular velocity. But this is a statement of a physical law about the behavior of watch springs. Moreover, if we say that a certain phenomenon lasts one hour, we do not mean to say that we can use only a spring watch in order to check the statement. We may as well use a pendulum clock and define one hour as a duration of a certain number of oscillations of this pendulum. But this is only possible if we assume the validity of a physical law: The unwinding of a spring as an effect of its elasticity proceeds at a rate which is proportional to the frequency of the pendulum as an effect of gravity. Since these and similar laws are valid, the expression 'one hour' defined by spring watches or pendulum clocks is helpful in giving a simple description of the actual motions of material bodies. Briefly, an operational definition is "simple and practical" if there are physical laws according to

which the numerical result of this definition is identical with the result of other independent operations. Or, in other words, an operational definition is "practical" if it plays an important role in the simplest formulation of valid physical laws.

We must appreciate this fact if we wish to appreciate justly the merits of "conventionalism" in physics and in science in general. Let us consider an example which has been used over and over again for stating the case of conventionalism: the principle of the conservation of energy. We start from the case that only mechanical and heat energy play a role. If we consider an isolated system, we can state: The sum of heat energy H and mechanical energy M remains constant through all interchanges. Mathematically: $H + M$ = Constant. This is certainly no longer true if electromagnetic phenomena also come into play. Then we have to introduce also an electromagnetic energy E, and the principle of conservation of energy would say $H + M + E$ = Constant.

The advocates of conventionalism argue: If the last equation is not confirmed by experiment, we can always add a term U in such a way that the equation $H + M + E + U$ = Constant is confirmed by our experiments. Then this last equation is the operational definition of the term U (unknown kind of energy). However, we note that the electromagnetic energy E has not been defined by our equation of conservation only. There is also a different operational definition of E.

It can be calculated from the electric and magnetic field intensity. Thus we have two operational definitions of E. To say that both these definitions render one and the same result means to assert the validity of a certain physical law. This law gives a practical value to the introduction of the symbol E. The introduction of the unknown energy U would be practical only if we knew a second operational definition of U which is independent of the conservation equation $H + M + E + U$ = Constant. Then the principle of the conservation of energy would no longer be purely conventional but a statement about facts.[15]

4. Misuse of the Vernacular of Physics

In some philosophical discussions the concept of energy has been used in a rather loose way. Speaking of biological or psychological phenomena, quite a few authors have argued as follows: If we examine the phenomena in living organisms, it may be that the sum of the physical and chemical forms of energy remains constant during the living process. If this were the case, it would confirm the hypothesis that life is a physico-chemical phenomenon. Let us now discuss the possibility that the physico-chemical forms of energy would not fulfil the law of the conservation of energy, that, e.g., a living organism would do more work than corresponds to the energy it has absorbed. Even in this case one could uphold the principle that the sum of

all forms of energy remains constant. One has only to introduce a new term L (energy of life or vital energy) into the law of conservation. If we denote the sum of all physical and chemical energies by P, we confirm by experiments that P does not remain constant during the phenomena of life. But if we choose L in a suitable way, we can always achieve that $P + L =$ Constant. In this case life could not properly be called a physico-chemical phenomenon. But nonetheless it could be treated by an exact science, a "vitalistic biology", which would contain a law of conservation of energy: The sum of physico-chemical energy and vital energy remains constant.

But in this vitalistic or organismic biology the operational meaning of L (vital energy) is defined by the conservation law ($P + L =$ Constant.) This law would state a "convention" as to how to use the word "energy of life L" and would be by no means a law about the physical world. The crucial test of the vitalistic biology is whether we can introduce, besides the state variables of physics and chemistry (mass, electric charge, etc.), a small number of biological state variables from which the energy of life (L) can be computed by a simple formula. Certainly we do not know of any attempt to do so. We have no "practical" operation to define the energy of life.[14]

By comparing the concept of energy in physics with this useless concept of vital energy, we learn to understand the real scientific meaning of the energy law in physics. We know that in physics the conservation law is not the only operational definition of electric energy; or, in other words, there are other equations in the calculus of physics besides the energy equation which contains the symbol E. In addition to it, the number of state variables upon which the energy law depends is a small one. We shall take up this last argument again in the discussion of the concept of force (sec. 15).

5. Mathematical Description or Physical Explanation?

Frequently a distinction has been made between two types of physical theories. One type starts as we did from a set of equations. This type of theory, we have been told, describes the observable phenomena. It is called, therefore, a phenomenological theory or a mathematically descriptive theory. But there is also, according to the traditional distinction, a second type of theory. Here the physical facts are not only "described" but also "explained". If we treat, e.g., the phenomena of light by a theory of the second type, we present a mechanical model of the ether which transmits light and a mechanical model of the atom which emits light. In this way we have a "causal" theory; we are given the causes of the light phenomena, not only a description. This type of theory is sometimes called a theory which uses pictures instead of equations—a pictorial theory. We have also been told frequently that only

this type of theory provides an understanding of nature and gives real satisfaction to the mind of the physicist. We have also been told that only this type of theory is really helpful in finding new facts, while the phenomenological theory is only helpful in recording facts which are already known.

As a matter of fact, the whole distinction between the phenomenological and the causal theory has been much overworked and overrated. If we are given a mechanical model of the ether, we have to formulate the equations of this model. And, possessing these equations, we have to proceed exactly as we did in the mathematically descriptive theory. The only thing which would clearly distinguish the second type of theory would be the requirement that these equations are to be of a particular form. They ought to be derived from the equations of motion, as formulated by Newton to handle the motions in the planetary system. But these equations themselves constitute the phenomenological or "descriptive" theory of a particular domain of physical phenomena, the motions of medium-sized material bodies. Therefore, the requirement that all physical phenomena should be explained by mechanical models would mean assuming that the motion of the smallest particles is covered by the same equations that have been extracted from the motions of medium-sized bodies. This assumption is, of course, far from being self-evident and, for that matter, has turned out definitely to be an oversimplification. We know very well that the motion of subatomic particles cannot be covered by Newton's laws of motion and therefore by no mechanical model. The statement that the laws of "modern" physics (e.g., relativity theory) are not "causal" laws but only "descriptive" ones has a definite operational meaning only if we use the phrase "causal explanation" in the sense of "derivable from Newton's laws of motion".[15]

6. The Metaphysical Basis of "Explanation"

This definition of "causal explanation" would make sense only if we ascribed to Newton's laws of motion a particular logical status. But doing so would mean a discrimination against any attempt to set up a new foundation of physics. The expression "a physical phenomenon is explained" means in the everyday language of the physicist: "The statements about this phenomenon can be derived from a set of equations and practical operational definitions from which we can also derive the bulk of the physical phenomena which are actually known." To single out a particular type of equations as the only legitimate basis of explanation is not justified by science. All attempts at such a justification have been based upon metaphysics. They have had their roots in the belief that the validity of some equations (e.g., Newton's equations) can be established "philosophi-

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cally" or "epistemologically" without reference to the observable facts which have to be derived. Otherwise the derivations from Newton's equations would no more be an "explanation" than the derivation from any other set of statements.[16]

Therefore, the distinction between a description and a causal theory is a purely metaphysical distinction. If we look back into the history of science, we notice that Francis Bacon, the leader of empirical philosophy, accused the Copernican system of being purely descriptive while, according to him, the old Ptolemaic system provided understanding.

Philipp Lenard and quite a few "empirically minded" scientists of the twentieth century accused Einstein's theory of relativity of being purely "descriptive", while Newton's theory was supposed to be causal and explanatory. It is instructive to take note of this similarity to the attitude which traditional philosophers took toward the advance in science. The only apparent difference is a shift in the content of the metaphysical creed. In the sixteenth century a physical phenomenon was said to be "explained" if it could be derived from Aristotle's philosophy, while in the nineteenth century only those phenomena were regarded as "explained" which could be derived from Newton's philosophy. This belief in the exceptional logical status of a particular type of equation had become, by-and-by, so firmly established that its metaphysical foundation was forgotten or driven into the subconscious. But the content of the creed remained alive and was more and more credited to "common sense" or "immediate intuition of nature". The empiricist Francis Bacon did condemn Copernicus, and the physicist Lenard did condemn Einstein, not in the name of metaphysics, but in the name of good sense or "unbiased and strictly empirical description of nature." Even a slight glance at the history of scientific thought shows us that the content of yesterday's metaphysics is today's common sense and tomorrow's nonsense.[17]

7. Pictorial Theories

It has been maintained frequently that the theories using the mechanical models are more "visualizable" or "pictorial" or "intuitive" than the theories starting from equations. I think that this claim has a very weak foundation. Speaking from a purely mathematical point of view, every description by an equation is equivalent to a description by a visualizable diagram or picture. But it seems that the philosophers and scientists who discriminate in favor of pictorial theories mean by this word "similar to a familiar group of sense experiences". As a matter of fact, the mechanical interaction of medium-sized material bodies is a phenomenon which we observe again and again in our daily life. These phenomena are covered by Newton's equations in a very satisfactory way. Every theory which describes new thoughts by comparing

them with these familiar phenomena strikes a friendly sounding chord in our minds. Such a theory seems to us intelligible and visualizable for this psychological reason. But to require that every theory should be set up according to this pattern would mean to assume that the laws governing this particular domain of phenomena are sufficient to cover all phenomena of physics, even the motion of the smallest particles with the greatest speeds. If this assumption is once rejected, what would be the point in calling a theory "not visualizable"? It would mean that the theory contradicts statements which have been dropped anyway because of their inadequacy.

A particular difficulty in understanding the meaning of physical theories has arisen from the confusing of a theory which is intuitive or visualizable with a theory which is supposed to be supported by our "inner intuition". Quite a few philosophers have asserted and quite a few scientists have agreed that, e.g., the axioms of geometry can be recognized as true by looking into our own minds. In order to give to this ambiguity the appearance of unambiguity, a particular word has been borrowed from German philosophy and introduced into the English vernacular of philosophy, the ambiguous word "anschaulich", which means "conceivable by our sense observation", but also "conceivable by the efforts of our minds even if not observable at all". These two meanings contradict each other in the application of that word to every actual problem.

III. Classical Newtonian Mechanics

8. Domain of Newtonian Mechanics; Mass

Classical mechanics or Newtonian mechanics is still the cornerstone of all physics. By this theory a group of phenomena is covered around which the research work of science was centered in the period when Galileo started the revolution which gave rise to modern physics. These phenomena are the motions of "medium-sized" material bodies, such as the motion of running cars, of machines, of falling and rolling stones. Newton set up a pattern of description which was adjusted as well to the motion of planets as to the motion of these medium-sized bodies. If we understand Newton's laws of motion which define this pattern, we have made the first step toward understanding physics. A great deal of misunderstanding of the recent physical theories (relativity and quantum theory) is due to the fact that Newtonian physics has been frequently misrepresented. There has been, in particular, a tendency in the teaching of physics to suggest that Newton's laws are almost self-evident. In quite

a few textbooks the authors express their bewilderment that such obvious truths have been ignored for such a long time. This kind of presentation is a serious obstacle in the way of a real understanding of physics. One has to emphasize, rather, how great a power of imagination was necessary to set up these laws, how long and strenuous the road has been which has led scientists from the observed facts to Newton's laws of motion. The more self-evident these laws appear to us, the less do we understand them.

For medium-sized material bodies we can tentatively define the "mass" of a body by determining the number of grams which we obtain by reading its weight on a spring-balance at sealevel. This operation is unambiguous, as the weight does not change if we move the balance to any point at sealevel. We note here again that the applicability of an operational definition is based on the physical fact that several operations render an identical numerical result. In the domain of the phenomena which are covered by Newton's laws of motion the following statement holds:

If we impart to a body of a given mass in a well-defined environment a certain initial velocity, its future motion is determined. If the size of the body is small, this motion can be described approximately by a curve. Let us now increase the mass, but not the size, of our body more and more without altering any of the other circumstances and examine how the orbit is altered by this fact.[18]

9. Field of Force; Gravitational and Electromagnetic Fields

The environment which has an influence upon the orbit is called the "field of force". It can be of two kinds. In the first case, the orbit is not at all altered by the increase of mass. An example of this case is the orbit of a projectile launched inside a vacuum. Then we call the environment "static gravitational field of force." By 'static' we mean that the field is due to masses which are all at rest (relative to the fixed stars). In the example of the projectile, for instance, the motion of the earth has to be neglected.

A typical example of the second case is the static electric or magnetic field of force. Its influence on the orbit of a charged particle becomes less and less the greater the mass becomes. We may consider, for instance, the path of an electrically charged particle moving through an electrostatic field which has a direction which is different from the initial velocity of the particle. If the charge remains constant while the mass of the particle increases, the particle will be less and less deviated by the field from its original direction. If the mass becomes "infinitely great", the particle will travel along a rectilinear path with constant speed whatever the direction of the field may be. The motion of an infinitely great mass is called an "inertial" motion, for this path is a result of mere inertia and independent of the field of force.

It is obvious that a rectilinear mo-

tion relative to a system of reference S cannot be a rectilinear motion relative to other systems S' which are accelerated or rotational in motion relative to S. To be unambiguous, we have to qualify our statement about the path of infinitely great masses. Only if we describe its motion relative to a particular system of reference S is the path of a very great mass in a field of force of the electromagnetic type a straight line and its speed constant. Such a particular system S is called an "inertial system". Our first superficial experience makes us believe that our earth is an inertial system. But more thorough investigations (of the kind of the Foucault pendulum) show us that the earth is in rotation relative to the particular system S and that rather the system of fixed stars should be taken as an inertial system, at least approximately. If we describe the path of an infinitely great mass relative to the rotating earth, it would be a kind of helix. For each system of reference it would be a characteristic curve. The inertial system is recommended only by the simplicity of this curve (straight line).

10. Newton's First Law of Motion (Law of Inertia)

These statements about the path of infinitely great masses are the factual content of what is known as the "Law of Inertia". Obviously, this law is empirical and cannot be derived from any self-evident principle. The only element in it which looks self-evident is the assertion that the straight line is

the "simplest" curve. But actually it is an observation statement, too, except that it describes psychological facts, not physical ones.

The Law of Inertia has an operational meaning only if the inertial system is described by a physical operation. As a first approximation we can say that S is identical with the system of fixed stars. Since a particular physical object (the fixed stars) enters into this law, it cannot be a self-evident statement. It is even clear that the Law of Inertia, if given this operational meaning, cannot be exactly true. For the fixed stars have their "peculiar" motions and do not form a rigid frame of reference. Therefore, the Law of Inertia in this form can be only approximately true. Einstein's general theory of relativity and gravitation attempted to replace the old Law of Inertia by a law which takes into account these peculiar motions of the fixed stars. Instead of stating that a mass travels along a straight line with constant speed, we can also state that its "acceleration" is zero. ('Acceleration' means change of speed or direction.)

If we consider a finite mass, we notice by experiments that in the same environment the smaller the mass, the greater the acceleration. An electrically charged particle moving in an electric field is the more deviated from its original course the smaller its mass. (The charge belongs to the environment.) We can state this experimental fact quantitatively by stating that the product mass times acceleration re-

mains constant if the environment does not change. Mathematically: If we denote the mass by 'm' and the acceleration by 'a' and put the product ma equal to f, the quantity f depends upon only the distances of the moving mass from the bodies of its environment.

The law $ma = f$ is known as "Newton's Second Law of Motion". We call f the "force acting upon the body m" and exerted by the bodies of the environment. If all this is true, we can derive a new operational definition of mass: The ratio of two masses is inversely proportionate to the accelerations which they get from one and the same force. This definition is only unambiguous if we obtain the same value of mass whatever force we apply. Again a physical law has to be regarded as valid in order to insure the identical result of the operations which define mass.

11. Heavy Mass and Inert Mass

The values of masses obtained from our original definition (by weighing) and our new one (by the ratio of accelerations) are identical. This is an important physical fact, too. We call "heavy mass" the result of weighing and "inert mass" the ratio of accelerations. Then we can state the law of the identity of heavy and inert mass. If we use this law, we can apply the relation $ma = f$ also to motions in gravitational fields, although it would seem that in this case the acceleration is independent of the mass. Here we have to assume that the force of gravita-

tion is proportional to the heavy mass m. The force f is equal to mg, where g is independent of m. Then $ma = f$ becomes $ma = mg$ and $a = g$. The acceleration a is independent of the mass m and is not altered if the mass becomes infinite. In this way Newton treated the gravitational field as an example of the general field of force. He advanced the hypothesis that the gravitational force is proportional to the inert mass m. This hypothesis is another way of stating the identity of heavy and inert mass. However, we must not fail to understand that the operational meaning of the terms 'inertia' and 'force' in the gravitational field is a particular one. An electromagnetic force produces an acceleration; this means a departure from the inertial motion, while in the static gravitational field the inertial motion (the motion of an infinitely great mass) itself is accelerated. The definition of force by "acceleration" and by "departure from the inertial motion" give now different results. A launched projectile is not deviated by a force if a "force" is defined as causing its departure from the motion of an infinitely great mass.

12. Newton's Second Law of Motion

Discussing Newton's Second Law of Motion ($ma = f$), the question has been raised frequently whether this law states physical facts which can be checked by experiments or whether it is only a definition of the term 'force'. Evidently, whatever may happen in the world of our experience, we can al-

ways introduce a force f which obeys the relation $ma = f$. Whenever there is an acceleration a (which is an observable quantity), we may assume the existence of a force f which cannot be observed itself but has to be calculated from the definition $ma = f$. Therefore, this "law" seems to be only a definition of the term 'force' and can be neither confirmed nor refuted by experiments. However, it is obvious that certain physical effects must be confirmed to make $ma = f$ an unambiguous operational definition of f. The product ma has to be independent of the mass m and has to depend only on the situation of the moving body in its environment. Then the law of motion says that equal masses have, under equal relations to the environment, equal accelerations. But the factual content of this law is very poor unless an operational definition of the term 'equal relations' is given which does not contain the acceleration produced.

The most famous example which decided the success of Newton's laws of motion was his theory of gravitation. The relation to the environment is determined in this case by the masses M of this environment and the distances r of the moving mass m from these masses. Every mass M contributes the term $f = Mm/r^2$ to the force. If we substitute this expression in the law $ma = f$, we obtain a law which contains only observable quantities. By using the "calculus" (Newton's equations of motion), the motion of our mass m relative to the inertial system

can be calculated. This theory has succeeded in giving a very good account of the observed motions in the planetary system.

However, this success cannot be interpreted as a confirmation of the law $ma = f$ by itself but is a confirmation of this law plus the law of gravitation Mm/r^2. The meaning ascribed by Newton and the Newtonian school to the law $ma = f$ itself has been the following: Since the substitution of Mm/r^2 turned out to be such a success, it seemed probable that every motion in the universe could be described by substituting for f in $ma = f$ a formula which is in a certain way analogous to Mm/r^2. But it may contain a different power of r or even a different function of r or even the velocity of m besides the distance. The main point was that f should be expressed by a simple function of the variables which determined the relation of m to its environment.

13. Inertial Force and Centrifugal Force

Newton's Second Law of Motion, $ma = f$, where f is the gravitational or electromagnetic force, holds only if a is the acceleration with respect to an inertial system (e.g., the fixed stars). If we want to calculate the acceleration a of a mass m relative to a system which has an acceleration or a rotation relative to the fixed stars, we have to use a modification of Newton's laws. We may call the rotating or accelerated system the "vehicle" (comparing it to a railroad car). The ac-

celeration of our mass m relative to the vehicle may be denoted by 'a_{rel}' and the acceleration of the vehicle (speaking exactly, of the point of the vehicle coinciding with the mass m) by 'a_{veh}'. If both these accelerations have the same direction as the acceleration a of our mass m relative to the fixed stars, we have obviously the equation: $a_{rel} + a_{veh} = a$. Newton's equation becomes then: $ma_{rel} = f - ma_{veh}$.

This means: The acceleration relative to the vehicle can be calculated as if it were at rest (relative to the fixed stars), except that we have to add to the electromagnetic or gravitational force f a new kind of force $f_{in} = -ma_{veh}$ which we call "inertial force". It increases proportionally to the acceleration of the vehicle but has the opposite direction. The simplest example is the shock which we feel when a railroad car is starting or stopping. a_{veh} is the acceleration of the car. If the vehicle is rotating, we can no longer use our simple relations between the accelerations.

But these relations still hold for the components of the accelerations in a certain direction. If we choose a direction perpendicular to the axis of rotation, $f_{in} = -ma_{veh}$ becomes what one calls "centrifugal force". It follows that the acceleration a_{rel}, perpendicular to the axis of rotation, can be calculated by adding to the radial component of f (electromagnetic or gravitational force) the centrifugal force. There is sometimes a confusion about whether the centrifugal force is really

a "force". The answer is simple: according to the original Newtonian definition ($ma = f$), only forces of the type of gravitational or electromagnetic force can be substituted for f. The centrifugal force does not produce any acceleration with respect to the fixed stars. But if we want to describe motions relative to a rotating system, we have to add the centrifugal force f_{in} to the gravitational or electromagnetic force f.

14. The Law of Force Must Be "Simple"

If the force f is not a simple function of the distances, masses, etc., the law $ma = f$ loses its factual content. This becomes obvious if we consider what has been the most outstanding achievement of Newton's laws. We remember that Copernicus described the planetary orbits by circles, Kepler by ellipses. But if we examine the departures from the earth's elliptic orbit produced by the attraction of other plants, by Jupiter, Mars, etc., we obtain, as the effect of these perturbations, curves of an extreme complexity. Newton, however, succeeded in describing these same orbits by the simple statement that the acceleration with respect to the fixed stars is a sum of terms each of which has a simple form Mm/r^2.[19]

This means: A very complicated curve or a very complicated mathematical function can be derived from the extremely simple function Mm/r^2. If this expression of force were as com-

plicated as the equation of the curves performed by the planets, there would be no point in replacing the geometrical description by the dynamical description. The hope of the Newtonian school has been to find for every type of motion a particular law of force of a simplicity similar to the law of gravitation—a law for the forces of cohesion, of chemical affinity, etc. This hope has been partly disappointed but partly also more than fulfilled. Since the turn of the century (1900) it has become more and more evident that the motion of the smallest particles (electrons and nuclei) cannot be described adequately by the Newtonian pattern. In the mechanics of subatomic particles (quantum mechanics) the term 'acceleration of a mass at a certain point in space' does not occur at all. Therefore, the concept of "force" loses its original operational meaning.

But, on the other hand, within the domain in which Newton's pattern of description can be applied at all (medium-sized bodies) not so many different laws of forces have been needed as Newton's school had expected. Besides the gravitational force, which is not even a force in the full operational meaning of this word, only electromagnetic forces have been used in twentieth-century physics—in the whole domain of phenomena which are covered by Newton's laws of motion. There are no special forces which are responsible for the phenomena of cohesion or of chemical valence. The force f in Newton's law $ma = f$

can always be calculated from the laws of electromagnetism and has therefore also an operational meaning which is independent of its definition by $ma = f$. Partly, however, these phenomena of chemical affinity and physical cohesion have to be treated by laws which are different from Newton's laws of motion. We are going to learn later that in these parts of physics "forces" are not defined by acceleration. This holds, in particular, for the phenomena of nuclear physics.

In engineering physics some laws of force are used which are "rules of thumb". In aerodynamic engineering, for instance, a law is used which says that the resistance of air which slows down the speed of an airplane is proportional to the square of its speed. But the physicists believe that this law can be derived from the laws of collisions between the molecules of the air and the molecules of the plane. The forces which are responsible for the effects of these collisions are the electric attractions and repulsions between the electric charges of which ultimately the atoms of gases and solid bodies consist.

15. Do Forces "Exist"?

These considerations will help us a little toward appreciating some attempts which have been made to apply Newton's laws of motion to living organisms by a kind of short cut. Quite a few authors have suggested introducing "vital forces" or even "spiritual forces". This would mean

substituting these forces for the symbol 'ƒ' in Newton's laws. We have seen that Newton's laws of motion have been a success in physics only because it has been possible to substitute for 'ƒ' an expression which is simple and has a certain similarity to the law of gravitation. If one had to introduce "entelechies" or "holistic tendencies" as state variables instead of distances and masses, the whole meaning of Newton's laws would be lost. We would be faced by a completely new type of laws. To apply Newton's laws in their real scientific meaning to the motions of living organisms means to assume that the forces determining these motions are not very different from the electromagnetic forces if we remain in the domain of bodies of such a size that terms like 'acceleration' and 'velocity' have an operational meaning. A spiritual theory of the living organism perhaps does not contradict Newton's laws but would make them a definition of the term 'force' and turn them into tautologies. The opinion that such vital or spiritual forces can be introduced into Newton's laws has its source in some connotations of the term 'force'. Since this word is used in our everyday language as a term of psychology, the impression has been produced that by virtue of this term a "psychical" element has a legitimate place in physics.

As a remainder of the organismic philosophy of science which was prevailing in the Middle Ages there has been a widespread reluctance to accept the operational definition of force by which force is defined in terms of motion.

It has been argued that in a stretched piece of rubber there is a force, a tension, even if the rubber is at rest. Therefore, we are told, 'force' cannot be defined by motion and does exist as an entity independent of motion. But the statement, "Force can exist without acceleration", is a very misleading way of stating the facts. It is in flagrant contradiction to Newton's definition of force. What really happens in the stretched rubber is a state of equilibrium between several forces. The algebraic sum of the forces acting upon any particle of rubber is zero. To say that forces "exist" in this rubber would be as correct as to say that in the number zero the number five "exists" because five minus five is equal to zero. The existence of a tension or force within the rubber means in terms of operations that, by removing a part of the rubber, the equilibrium is disturbed. Acceleration of the rubber particles is produced and the force reveals itself. By saying that this force has existed before and was only balanced by other forces, we do not add anything to the operational meaning of our description of facts.

The only logical, sound way of putting the problem of the "existence of force" is again to avoid what Carnap calls the "material mode" of speaking and to stick to the "formal mode." We have to ask: If we set up a system of axioms from which Newtonian

mechanics can be derived, is it necessary to introduce 'force' as an undefined term? The answer is clearly in the negative. We can introduce 'force' as a term defined by acceleration if we assume the validity of some statements about physical facts.

The insistence upon the use of the term 'force' as an "entity of its own" has its sources only in some psychological connotations of this word. We can quote the example of the Nazi philosophy in which the word 'force' is regarded as dear to the mind of the Nordic race. Every attempt to introduce a definition of 'force' by motion is branded as an act of the enemies of the Nordic race.

However, it is possible to give an operational meaning to the statement that "forces are physical realities". According to P. W. Bridgman, a term of physics describes a physical reality if several independent operational definitions of this term can be given which render one and the same numerical result.[20] If in a specific case a force cannot only be defined by acceleration but also as a function of masses, distances, etc., we have two independent operational definitions of force. We can say in this case that the term 'force' refers to a physical reality. This is obviously the case for the gravitational and electromagnetic forces which can be calculated from masses, electric charges, etc. But it is obviously not the case for spiritual and vital forces which cannot be calculated from observable data.

IV. Heat, Irreversibility, and Statistics

16. Recoverable and Irrecoverable Changes of State

If a small part of the physical world passes from a state s_0 into a state s_1, the following question arises: Is it possible that in this part of the world the original state s_0 be restored without changing anything in the rest of the world? If this restoration is possible, we call the transition from s_0 to s_1 a 'recoverable transition'. If not, we speak of an 'irrecoverable (irreversible) change of state'. To understand the role of irrecoverable phenomena and in particular the phenomena of heat, we are going to consider a simple example.[21]

The part of the world which we consider may be a perfect gas which is isolated from any supply or loss of heat. But we can change the volume of this gas by moving a piston closing the container of the gas. The initial state s_0 is a certain volume V_0 and a certain temperature T_0 of our gas, e.g., a volume of 2 liters and a temperature of 0° centigrade. The second state s_1 may be the result of an increasing pressure upon the piston. The final volume may be V_1 (e.g., 1 liter). We perform the compression by putting only enough weight on the piston to make the pressure exerted just a little greater than the expansion pressure of

the gas. Then the transition from the initial volume V_0 to the final volume V_1 will occur infinitely slowly. Since the gas is, during the whole transition, almost in equilibrium, we speak of a quasistatic change of state. This means that the process is almost static. As no heat is lost, the temperature increases by the compression. When the gas has reached the final volume V_1, the temperature has increased to a value T_1, which is greater than T_0. According to an easy calculation, this temperature can be calculated by the formula: $T_1 V_1^{k-1} = T_0 V_0^{k-1}$ (k has the value 1.4 for a diatomic gas like oxygen).

Outside our gas the effect of the process has been the lowering of a weight which has been pressing upon the piston. Hence, the effect on the whole world has been lowering of a weight, increase in a temperature, and decrease in the volume of our gas. Is this change of state from s_0 to s_1 recoverable? Can the state s_0 be restored? Certainly, yes. We have only to reduce slowly the pressure upon the piston. Then the gas will expand again infinitely slowly because the pressure on the piston is just a little smaller than the internal pressure of the gas. When the original volume V_0 is restored by expansion, the weight on the piston is in its original position and the initial low temperature T_0 is also restored. This means that the change of state from s_0 to s_1 is really recoverable.

Let us now modify our change of state. We start again from s_0 but produce a change of state in a new way. The gas may remain isolated against

heat supply from outside. Now we increase its temperature. But we do it at constant volume V_0. We can, e.g., like in the old Joule experiment, have a paddle wheel rotating in the gas. The rotation can be produced by the fall of a weight which is connected with the wheel by a cord over a pulley. The gas will be heated by friction. The state s_0 will be changed into a state s_2 which has the same volume V_0 but a temperature T_2 which is greater than T_0. Is the change of state from s_0 to s_2 now also recoverable? Can a change of state occur which restores s_0 if no heat is supplied from outside? This change would mean that the weight which produced the friction by falling has been lifted again, while the temperature of the gas which had increased by friction has dropped again. The final result of the change of state from s_2 to s_0 would be the decrease in temperature of the gas and the lifting of a weight outside the gas, while nothing else has changed in the world.

If we generalize the results of all our experiences on the conversion of heat into work, we notice that a weight can never be lifted just by using the heat supplied by the drop in the temperature of one single reservoir. This generalization has become a basic hypothesis of physics. It is usually called the "Second Principle of thermodynamics" (the First Principle states the conservation of energy). From this Second Principle it follows that the change of state from s_0 to s_2 is not recoverable. This means: The increase of temperature by friction at constant volume is

not recoverable. It is obvious that by this change of state from s_0 to s_2 (friction) the function TV^{k-1} increases as V remains constant and T increases.

17. The Second Principle of Thermodynamics and Entropy

The function $F = TV^{k-1}$ increases by the process of friction and would decrease if a weight were lifted and nothing else happened than the decrease in the temperature of one body (e.g., our gas) at constant volume. The function $F = TV^{k-1}$ may, provisionally, be called "entropy". This entropy remains obviously unaltered as long as the process is quasistatic and no heat is supplied. The entropy increases if a change of state takes place which is irreversible. The entropy would decrease during a change of state which is forbidden by the Second Principle of thermodynamics. This principle can therefore be formulated for a perfect gas as follows: There is a function F of the state variables V, T with this property: if no heat is supplied, F can only increase. It remains constant during a quasistatic process. But this is only a limit which can never be reached by an actual experiment.

If we pass from a perfect gas to a general system, we can formulate the Second Principle in an analogous way. The whole system consists of several bodies with the volumes V_1, V_2, etc., and the temperatures T_1, T_2, etc. Then there is again a function $F(T_1, T_2, \ldots, V_1, V_2, \ldots)$ of the state variables which behaves in a simple

way during changes of state which are not accompanied by an exchange of heat between these bodies and the outside world. The function F remains constant during quasistatic processes. These changes of state are recoverable with the approximation with which a quasistatic process can be performed at all. All changes of state which entail an increase of F are irrecoverable, while changes of state with decreasing F are impossible.

As a matter of fact, every function of F which is increasing with increasing F has the same properties as the "entropy". It turns out to be convenient not to introduce $F = TV^{k-1}$ but $S(V, T) = \log F$ as the "entropy" of a perfect gas. For if we consider two gases with the common temperature T and the volumes V_1 and V_2, we can show easily that during a quasistatic process

$$F(V_1, T) + F(V_2, T) = TV_1^{k-1} + TV_2^{k-1}$$

does not remain constant while

$$\log TV_1^{k-1} + \log TV_2^{k-1}$$

does. If we introduce the function $S = \log F$,

$$S(V, T) = \log TV^{k-1} = \log T + (k-1) \log V,$$

and call it definitely "entropy", we can assert a simple law. If S_1 and S_2 are the entropies of two gases which are in temperature equilibrium, the entropy of the whole system is $S_1 + S_2$. This means that $S_1 + S_2$ remains constant during quasistatic processes of the whole system.

The most popular example of increasing entropy is the passage of a system in which there are great differences in temperature into a system in which these differences are smaller. In the case that no heat is supplied from outside, one can show by an easy calculation that the entropy increases by the disappearance of these temperature differences. Therefore, a process of leveling temperature intensity in an isolated system is irrecoverable. Only in the borderline case of a quasistatic process does the entropy of the isolated system remain constant. The system performs recoverable changes of state. Therefore, the quasistatic processes are also called 'reversible processes'.

18. Cosmological Implications

All these facts and generalizations have been used to predict a very gloomy future for our world. The Second Principle of thermodynamics has been formulated as follows: The entropy of the universe tends toward a maximum, and this maximum is attained if the whole world is of constant temperature. Since the world is an isolated system, every change of state is connected with an increase of entropy. When the world has reached the state of greatest entropy, no change is longer possible; the universe will "die".

In a similar way it has been argued that only a finite number of years have passed since the creation of the world. If the actual laws of nature had been valid through an infinite number of years, the maximum of entropy would have already been attained. Since this is obviously not the case and the world is still "alive", the laws of nature and our world itself cannot have existed for an infinite number of years. It must have been created a finite number of years ago.

Since all processes which are covered by Newton's laws of motion can be reversed, this tendency of the world toward the ceasing of all changes has been interpreted as a feature of nature which cannot be explained by mechanics. A kind of striving toward an end has been envisaged by many philosophers. It has been cheered as a spiritual factor. This running toward the death of the world has been interpreted as being in contrast to the evolution of organisms which shows a tendency toward greater and greater differentiation. Two conflicting tendencies must therefore be recognized in the universe both of which cannot be explained by mechanical laws. Sometimes this conflict has been interpreted as a scientific background for the eternal struggle between God and the devil.[22]

As a matter of fact, the term 'entropy of the universe' is, as P. W. Bridgman says correctly, a pure "paper and pencil affair". We can, of course, say that the entropy of the universe is the sum of the entropies of all parts of the universe. But speaking exactly we do not know whether the expression 'entropy of the Universe' has any operational meaning. For the sum of the entropies of the parts of a

system can be regarded only under very restricted conditions as the entropy of the whole system. We do not even know whether the sum is convergent from the purely mathematical viewpoint. Therefore, all these conclusions concerning the creation and death of the world are results of a loose way of thinking.[23]

19. The Kinetic Theory of Heat

We shall understand the whole problem of recoverability much better if we attack the question from a different angle. We ask whether "irrecoverability" is compatible with the hypothesis that heat is a movement of small particles. Then we shall learn that irrecoverability is the result of a rather shortsighted view of the universe, while a more penetrating view would reveal to us a universal recoverability. We must never forget that the results of thermodynamics have operational meaning only under conditions which can be described by the concepts of thermodynamics: temperature, mechanical work, entropy, etc. But there are conditions which cannot be described by what P. W. Bridgman calls the "universe of operations" of thermodynamics. This fact is obvious from the simple consideration that in no statement of thermodynamics is the expression 'velocity' or 'time' used. Therefore, we are given not the slightest estimation of how long it may take to establish a certain state of the world. If a theory does not contain any statement about time, it has no operational meaning to say that the

theory predicts a certain future of the world. For the time may just as well be "never."

There are certainly phenomena of heat which are not covered by the laws of thermodynamics. In this category belong all phenomena of fluctuation like the Brownian movement, the spontaneous change of density in the atmosphere which accounts for the blue sky, etc. For this reason we are in need of another method to deal with the phenomena of heat. This method is provided by the "kinetic" or "statistical" or "molecular" theory of heat. Its basic hypothesis asserts that, besides the observable motions of bodies, there is an irregular zigzag movement of its microscopic and submicroscopic particles which on the average does not contribute anything to the observable velocity of medium-sized bodies. Moreover, according to this hypothesis, the average kinetic energy of this molecular motion is proportional to the absolute temperature of the body.

This hypothesis which accounts for a great many facts connected with the conversion of heat into mechanical work and vice versa faces a great difficulty if we apply it to the existence of irrecoverable changes in the universe.

20. Mechanics Cannot Account for Irrecoverable Changes of State

We consider a simple case. There may be a gas inclosed in a container. The gas molecules are performing rectilinear motions and are only deflected if they hit each other or the walls of

the container. We start from a state s_0 in which all molecules are assembled in a small corner of the container while most of its volume is empty. This means in terms of thermodynamics that the density of the gas in this corner is great while the density in the rest of the volume is zero. In the second state, s_1, the molecules may fill up the whole container with equal density. According to the rules for calculating the entropy, this physical quantity has in the state s_1 a much greater value than the state s_0 of our gas. As in an isolated system the entropy can only increase, the change of state from s_0 to s_1 is, according to the laws of thermodynamics, irrecoverable.

According to these laws, it can never happen that a gas which fills up a volume with constant density can undergo changes of density by which eventually the whole mass is assembled in one corner. However, according to the kinetic theory, we cannot understand why such a thing should be impossible. Let us assume that the state s_1 (equal density) has just been reached. Then we reverse the direction of the velocities of all particles. According to Newton's laws of motion, they must traverse now the same orbits as before but in an opposite direction, until they reach finally again the state s_0 and assemble in one corner. Therefore, the kinetic theory seems to be incompatible with the existence of irrecoverable changes of state. This means that the kinetic theory would be incompatible with the Second Principle of thermodynamics. For from mechanics it

seems to follow that a homogeneous gas can spontaneously move into a small corner.

This argument is conclusive. Newton's mechanics as the general Law of the Universe and the existence of irrecoverable changes are incompatible. The kinetic theory of heat cannot be based on mere mechanics. It needs, in addition to Newton's laws of motion, still a different type of hypothesis. The kinetic theory makes use of *statistical hypotheses*. L. Boltzmann was the first to point out how, by combining mechanical and statistical hypotheses, it can be predicted that the initial state of leveling processes like heat conduction and diffusion is "irrecoverable", if we take this word in its practical sense. However, it can also be derived from statistical hypotheses that this irrecoverability is only a superficial aspect. If we observe the phenomena more in detail and over long periods of time, we can bet that the initial state will once reappear.

21. Statistics and Irrecoverability

The statistical hypotheses, in contrast to the mechanical hypotheses, do not make an assumption about how a specific particle behaves at a specific time in a given field of force. Statistical assumptions say, in a certain sense, more and, in a certain sense, less. We explain their meaning by a simple example. We may divide the whole volume of our container of gas into a great many small cells. Our hypothesis says: If we pursue the path of a

particle over a long time (years or centuries), the time during which it dwells in a specific cell is a specific fraction of the whole time considered. The volume of the cell is in the same ratio to the whole volume of the container as the dwelling time in the cell is to the whole time considered. This means that this ratio becomes more and more independent of the time of observation. This ratio may be denoted by 'p'. If there are, e.g., 100,000 cells, p is a very small fraction (e.g., $p = 1/100,000$) and may be called the "dwelling time" in a specific cell. If we consider a second gas molecule moving independently of the first one, it spends again the $1/100,000$ part of any given time stretch in our specific cell. If T is the whole time of observation and T' is the dwelling time of the first particle in our cell and T'' the time during which both the first and the second particle are dwelling in our cell, we have

$$T' = pT \qquad T'' = pT' = p^2 T .$$

If we want to know the time during which N particles would all dwell in one specific cell, we would find that it is equal to $p^N T$. If we assume, e.g., that N is equal to one million (10^6), the dwelling time of all N particles in one specific cell would be

$$T \cdot p^{1,000,000} = T \left(\tfrac{1}{100,000} \right)^{1,000,000}$$
$$= T \frac{1}{10^{5 \times 10^6}} = T \frac{1}{10^{5,000,000}} .$$

The denominator is a number in which the digit 'one' is followed by five million zeros. Therefore, if we make observations during a billion years ($T =$ a billion) the average "dwelling time" of our particles in one particular corner will be much less than the billionth part of the billionth part of a second. Moreover, if we are the lucky observers of such an event, we can bet that it will soon disappear and not reappear for a billion generations to come.

This is the statistical aspect of irrecoverability: There are rare states of a system and they disappear very soon. Hence, we can bet in what direction the system will develop. But if we start from a very frequent state (e.g., from the state where the particles are equally distributed over the whole container), we can bet that it will hardly happen in a billion generations that the rare state will be restored in which all particles assemble in a particular corner.

The sequence "rare state–frequent state" happens as often as "frequent state–rare state". The illusion of irrecoverability in the realm of our observations results from the fact that we start every experiment from a "rare" state of our system and our time T is always short. Then we can bet that the future will bring states which are not so rare. As a result, essentially there is no asymmetry of sequence but a distinction between rare and frequent states of a system. Starting from a rare state, s_0, the next state will usually be a frequent one, while, starting from a frequent state, the next one will usually be again a frequent one.

If we pass from the special case of a gas to a general case, we may say that in every system we have to set up a statistical hypothesis which allows us to compare the different states of a system with respect to their relative dwelling times.

22. Statistical Hypotheses Are Assertions about Physical Facts

The kinetic theory of heat is based upon statistical hypotheses which make predictions about motions of particles as the mechanical hypotheses do. They could, therefore, contradict Newton's laws of motion. In setting up these statistical hypotheses, their compatibility with Newton's mechanics has to be assumed as a special hypothesis. Every statistical hypothesis assumes a specific regularity about the dwelling time of particles in a certain domain. The essential in "Boltzmann's statistics" is the assumption that the dwelling time of a particle in any specific domain is independent of how many other particles are present in this domain. If we drop this hypothesis, we alter substantially the conclusions drawn from the kinetic theory. In the modern wave mechanics, which has replaced Newton's mechanics, Boltzmann statistics have been replaced by other statistical hypotheses. For each particle a particular statistical hypothesis has been set up. For some particles (e.g., photons) the dwelling time of the particle is increased by the presence of other particles (Bose statistics). In

other cases (e.g., electrons) the dwelling time is unfavorably influenced by the presence of other particles (Fermi statistics).

We must not be too puzzled by the fact that irrecoverability only comes into the picture if we start from a rare or improbable state of the system. But we may ask: How does it happen that we are living in a period and on a place of the universe where the entropy is increasing? We must always remember that the statistical approach to the happenings in the universe starts from the assumption that these happenings are in agreement with our basic statistical hypotheses. This means that the universe has already traversed a great many cycles. All the rare states have appeared and disappeared and will reappear again. This universe is, in the sense of ancient philosophy, an Epicurean universe. The origin of the sun, the earth, the elements, and even of our own human race is due to the law of chance. The appearance of men in the universe is a very improbable event from this viewpoint. In the Aristotelian universe, on the other hand, everything has developed according to plan toward a certain end of higher perfection. However, if we start from the assumption that our universe is an Epicurean one, our whole existence is due to chance, and we, the human race, are a very improbable event. Therefore, it is obvious that the universe will mostly go back to a state which appears more frequently according to the fundamental statistical hypothesis. But all

the rare states have their proper chance to reappear.

These considerations are pertinent for the attitude of philosophers toward the principles of thermodynamics. A great many philosophic interpretations of physics have made use of the principle of increasing entropy to bolster up an anti-mechanistic teleological view of the universe which invokes a tendency, a direction toward a certain end, instead of a causal chain of events. Unfortunately, the trend of

events, if we take thermodynamics for granted, tends toward destruction of the universe. If we, on the other hand, replace pure thermodynamics by the kinetic theory of heat (laws of motion + statistical hypotheses) we drop implicitly the Aristotelian theory of the universe and accept the Epicurean view that every tendency toward an end is an illusion and the real actor in the evolution of the world is a play of chances and the survival of the fittest. There is no real irrecoverability.

V. The Theory of Relativity

23. Einstein's Two Basic Principles

Newton's mechanics was applied to a hypothetical medium, the "ether", which was supposed to fill up the whole world-space. The wave motion in this ether was interpreted as being responsible for what appears to our sense observations as phenomena of light. The "ether theory of light" treats these waves according to Newton's laws of motion. The first result of this theory is that in empty space the waves of light are propagated with a speed $c = 3 \times 10^{10}$ cm/sec with respect to the ether. The speed of the source of light has no influence upon this speed of propagation. This result can also be formulated without speaking of the ether. One proceeds in a way which is similar to the replacing of Newton's absolute space by the "inertial system" (sec. 9). We say that in empty space the waves of

light are propagated with the speed c relative to a certain "fundamental system S".

Let us now examine phenomena due to the simultaneous motion of material bodies and propagation of light waves. Then Newtonian mechanics and optics lead to results which are not in agreement with the experimental facts and, moreover, are by themselves rather awkward. If we stick to the belief that Newton's laws of motion are the only legitimate basis of physics and that light is in particular a wave motion in a medium which follows these laws, we have to resort to complicated additional hypotheses.

In 1905 Einstein dropped both assumptions: Newtonian mechanics and the ether theory of light. His new theory of light and motion retained Newton's laws only for a limited class of motions which obviously follow these

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laws. These are the motions with "small velocities." This means velocities which are small in comparison to the velocity of light. Troubles arise only if the speed of material bodies approaches the speed of light. Further, Einstein retained two results of the ether theory of light which are made plausible by experiments. But he regarded these results as straight generalizations of experimental facts without troubling about whether they could be derived from the ether theory of light or from Newton's laws of motion.

Einstein's two hypotheses (or principles) are: first, the speed of light relative to any room is independent of the speed of the source of light relative to this room (principle of the constancy of light velocity); second, the propagation of light relative to the walls of a moving room is determined by the initial conditions relative to this room provided that the motion of this room is a rectilinear and uniform motion relative to the fundamental system S (principle of relativity).

This principle can also be formulated as a statement about the impossibility of performing a certain set of operations. If our room has the speed v relative to S, it says that there is no physical experiment by which we can measure this quantity v, i.e., the expression 'speed of a room relative to the fundamental system' has no operational meaning. If we substitute into Einstein's two principles the operational definition of 'speed of light relative to a system', 'speed of the

light source', etc., they become physical hypotheses about the interactions between the motion of bodies and the propagation of light. They are to be confirmed as any physical hypotheses by the agreement of their results with direct observations. These results are in conspicuous contradiction to the results derived from Newton's laws of motion in connection with the traditional ether theory of light. For this traditional theory allows for a particular operation by which this speed v can be determined (Michelson's experiment).

24. Newton's Laws of Motion Not Universally Valid

Actually, none of the observable phenomena derived from Einstein's two principles has been in disagreement with the results of observation. In this sense all statements derived from these principles are "true". Then it follows easily that some immediate results of Newton's laws of motion cannot be true. According to these laws, the increment of velocity produced by a given force is independent of the actual (initial) velocity Therefore, by the continuous action of a constant force, any increment of velocity could be produced. We could, e.g., obtain in this manner a velocity v of a body greater than the velocity c of light in a vacuum. But we can show easily that this result is in flagrant contradiction to Einstein's principle of relativity. Assuming the universal validity of Newton's laws, we

could consider a room which is moving with the speed of light relative to S. By a light source at rest in this room light is emitted through the vacuum toward one of the walls. This light will never reach this wall which itself moves with the speed of light. Therefore, no reflection can occur in this direction. By the absence of reflection we could make sure that our room has the speed c relative to S. But the determination of this speed is impossible, according to the principle of relativity.

Therefore the assumption that Einstein's principle is valid implies the negation of Newton's laws. We must assume that in contrast to Newton's mechanics the increment of velocity produced by a force is dependent upon the actual velocity. We can calculate exactly this dependence from Einstein's principles and obtain the result that this increment becomes the smaller the greater is the actual velocity, and tends toward zero if the actual velocity approaches the speed of light (the force being constant). This modification of Newton's laws can be checked by experiments. One can observe the increment of velocities produced by electric and magnetic forces if one examines fast-moving electrons, as cathode rays or the rays emitted by radioactive substances. These rays travel with a speed which is comparable to the speed of light. According to Einstein's results, the dependence of the increment of velocity upon these great initial velocities is great enough to be observable.

25. The Operational Definition of 'Mass' Has To Be Modified

This dependence has been confirmed by experiments. Hence some operations which are to render an identical result according to Newton's laws no longer do so. Let us consider the operational meaning of 'the mass of a particle'. If we assume that the field of force is known (e.g., given by Coulomb's law), we can get the mass by measuring the acceleration a (increment of velocity per unit of time). The mass m is defined by $m = f/a$. According to Newton's mechanics, the result is independent of whether the initial velocity was small or great. But if Einstein's principles are right, this operational definition becomes ambiguous. The acceleration a (and, therefore, m) depends actually upon from what initial velocity we start the experiment. In order to obtain an unambiguous result, we have to specify the operations involved, in particular the initial velocity v. If we require that the initial velocity be zero relative to S, the acceleration becomes unambiguously determined. We must therefore use a modified operational definition of 'mass'. We can either make the specification that the initial velocity relative to S is zero; then we define a concept which is called "rest mass" m_0. Or we can include the initial velocity v in the description of the operation. Then acceleration and mass themselves become dependent upon v. We obtain a physical quantity which is no longer a constant but a function of v. This

quantity is called "mass" m in the new mechanics. By using this definition, we can formulate the laws of motion in the simple form: mass times acceleration equals force ($ma = f$). But the mass m is now a function of v.

26. The Operational Definition of 'Time Distance' Becomes Ambiguous

If we assume that all observable results of Einstein's two principles are true, we have to assume that all results which can be logically derived from them are also true, even if they are not observable directly. Among these conclusions are several which have been exciting to a great many people. They have seemed to contradict common sense, in general, and the glorified common sense called "philosophical insight" in particular.

We can conclude, e.g., from Einstein's two principles that a clock which travels with the speed v relative to S loses time compared with the clocks at rest in S. If we recall the operational definition of a clock, we notice again that some operations which rendered, according to Newton's laws, identical results no longer do so if Einstein's principles are assumed to be true. The operations by which the time distance between two events was defined did not mention the speed of the clock relative to any system of reference. For, according to Newton's physics, this speed is without influence upon the march of the clock. If Einstein's principles are true, this operational definition of the time distance between two events becomes

ambiguous. We must specify the speed of the clocks used in this measurement. In order to obtain an unambiguous result of our defining operation, we must no longer say that "between the events A and B there is a time distance of 10 seconds" but that "there is a time distance of 10 seconds if we use clocks which are at rest in a particular system S'". We can express this statement a little more briefly by saying that "this time distance is 10 seconds relative to the system S'". The velocity of S' relative to S must be specifically given. We use again a "relativized language" in order to make the description and the operations unambiguous.

By starting from Einstein's principles, one can derive new laws of motion which differ for high speed from Newton's laws. We can in the same way derive laws for the propagation of light through material bodies which could be derived from Newton's mechanics and traditional wave optics only by complicated additional hypotheses.

The system of all statements which can be derived from Einstein's principles is called "the Theory of Relativity". This theory is so called because its characteristic and basic hypothesis is a principle of relativity. Before Einstein the validity of Newton's laws of motion and of the theorem of relativity for optical phenomena seemed to be incompatible. Einstein's master-idea was to drop Newton's laws of motion as well as the ether theory of light and to generalize

Newton's theorem of relativity into a general hypothesis which should be valid in the whole domain of the motion of material bodies and of light propagation.

27. The Relativity of Time

By people who are interested in the philosophical aspects of relativity theory the question has been raised: Has science really proved that time is relative and not absolute? The answer can be given quite directly.

The statement that "time is relative" can have two different meanings. First, it may mean that the march of a clock is altered by a rectilinear motion with constant speed v. This assertion is a statement about facts which can be checked directly or indirectly by experiments. It is confirmed indirectly by every experiment which confirms an observable result drawn from Einstein's principles. It is checked more directly by experiments which examine how the frequency of the light emitted by a sodium atom is altered by the motion of this atom (atomic clock, Ives's experiment).

But this physical meaning of the relativity of time is not the one which has puzzled the philosophers and the great public. 'Relativity of time' has a second meaning, a logical or philosophical one. From the theoretical and experimental results of Einstein's principles it becomes evident that the statement that "there is a time distance of 10 seconds between two given events" has no operational meaning.

We must always add explicitly a particular system of reference. Therefore, Einstein suggested dropping entirely sentences of the form "the time distance is 10 seconds" from the language of physics and of admitting only sentences of the form "the time distance is 10 seconds relative to the system of reference S". This suggestion is not a statement about physical facts which can be confirmed or refuted by experiments. It is a suggestion for using a language which is recommended as being well adjusted to our experience about physical facts.

Nobody can be persuaded to accept this recommendation if he does not like it. The adjustment of our language to the physical facts may not be the primary motive of our rules of language. Perhaps we want to adjust our language to tasks which are beyond our desire for a clear description of facts. If we wish, we can, of course, use the expression 'real time distance between two events' without referring to a particular system of reference. But we must know how to give to such a statement an unambiguous operational meaning. Either we concede that we do not know which of the arbitrary systems of reference is the "real" one and leave the discovery of this system to a superior spirit, or we single out some convenient system (e.g., the system of fixed stars) and give to the time distance relative to this particular system the honorary title "real or absolute time distance" while we call the time distance relative to other systems "relative or ap-

parent time distance". Nobody can prove that this way of speaking is "false". If we believe, e.g., that our moral values should be described by using the term 'absolute', one could find it convenient to use in the description of the physical world as far as possible the same terminology as in describing the goals of human behavior. Then it would be advisable for believers in "absolute values" to use also in physics the expression 'absolute time distance.'

28. The Speed of Light Has the Same Value in All Systems of Reference

Among the results of Einstein's principles which cannot be checked directly by experiments, the one which has been regarded frequently as its "central absurdity" is perhaps the following: Any light ray has, relative to S, the same speed as relative to a system S' which moves with an arbitrary speed v uniformly relative to S. Some philosophers have taxed this statement as being absurd or even self-contradictory. They argue: The speed of light relative to S is c. If S' moves with the speed v (which is smaller than c) in the direction of the light ray, it advances more slowly than the light. Therefore, the speed of light relative to S' is only $c - v$, which is obviously smaller than c. To assert that the speed of our light ray relative to S' is also c means to say that $c - v$ is equal to c while v is different from zero. This is obviously absurd. However, through

all this argument the velocities are measured by instruments at rest in S. Only if this operational definition of speed is used, can it be proved that the speed of light relative to S' is $c - v$. But in Einstein's statement 'velocity relative to S'' means the velocity measured by instruments which are at rest in S'. Then it cannot be proved that this speed of light relative to S' is $c - v$ but rather that it is c. The confusion comes from the failure to distinguish between mathematical symbols 'v' and 'c' and their operational meaning. Without the addition of the operational definitions, physical statements do not say anything about physical facts and cannot be checked by experiments.

If we have this in mind, we can understand that the statement that "no material body can move with a speed which is equal to or greater than the speed of light" is not a statement about absolute motion. It means, of course, that no material body can move with the speed c relative to S, but it means just as well that no material body can move with the speed c relative to S' (which moves in turn with the speed v relative to S). The real operational meaning of our statement about the speed of light as the upper limit of all speeds of masses is the following: If we have a set of clocks and yardsticks adjusted to a system S, no material system S' can move with the speed c relative to S if we measure c by the instruments in S.

29. The Traveling Twins

Another startling result is the story about the traveling twins. We learned that a clock which is moving with the speed v relative to S loses time compared with the clocks which are at rest in S. A human being is a kind of clock. The chief functions of the human organism, in particular the heartbeats, are achieved by periodical mechanisms. According to Einstein's principles, every periodical mechanism behaves like a clock. If we have twins, one at rest in S and one moving with the speed v relative to S, the traveling one will experience a smaller number of heart-beats. This can be seen particularly clearly if the traveling brother traverses a large circle which eventually leads him back to his stationary brother. Then the traveler must have remained younger than his brother at home. This conclusion is a necessary consequence of Einstein's principles. This result can, of course, neither be confirmed nor refuted by direct experiments. We never have to do with organisms traveling at a speed which is near to that of light without any violent disturbance produced by an acceleration. The possibility of this phenomenon can perhaps be understood if we consider the recent physiological experiment concerning the conservation of organisms at low temperatures. For a bath at a temperature well below the freezing-point brings about a slowing-down of the periodical processes inside the organism. It follows certainly from Einstein's two principles that the twin brother who has been at rest in the fundamental system S becomes older than his brother who performed a circular motion relative to this system.[24]

30. Conversion of Mass into Energy and Vice Versa

According to Newtonian physics, the sum of masses cannot be changed by any interaction of material bodies. According to the theory of relativity, the masses are dependent upon speed. However, the question arises whether the sum of the rest masses can be altered by interaction. From Einstein's principles it can be derived that the sum of the rest masses remains unaltered only if the sum of the kinetic energies is not altered by the interaction considered.

But if we consider phenomena which are connected with the conversion of kinetic energy into other forms of energy, e.g., into heat, the matter is different. As the simplest case we consider two bodies of equal rest masses which move with equal speeds relative to S in opposite directions. A collision takes place. If the bodies are perfectly rigid (unelastic), both bodies will come to rest in S and their whole kinetic energy will disappear and be converted into heat energy. It can be strictly derived from relativity theory that by this collision the sum of the rest masses of the colliding bodies is changed. Before the collision the rest masses of each body

were m_0; the rest mass of the whole system, $2m_0$. After the collision we have one larger body consisting of both small ones. Its rest mass will be greater than $2m_0$. From the theory of relativity it can be strictly derived that the increment of rest mass will be equal to the loss of kinetic energy divided by c^2 (the square of the speed of light). Since the loss of kinetic energy is equal to the produced heat energy H, we can also say that the rest mass after the collision is $2m_0 + H/c^2$. This is a special case of a more general theorem which follows from the theory of relativity: If by an interaction of material bodies kinetic energy E disappears, the sum of the participating rest masses increases by E/c^2. If kinetic energy E is produced by the interaction, the sum of the rest masses would decrease by E/c^2.

We can verbalize this fact by saying that rest mass is partly "converted" into kinetic energy. If the rest mass decreases by Δm_0, the kinetic energy $E = c^2 \Delta m_0$ is produced. The same thing is true if we replace production of kinetic energy E by production of radiant energy E. Continuing this line of argument, one can envisage the possibility that the whole rest mass m of a body could be converted into energy. Then the energy $E = m_0 c^2$ would be produced and the whole rest mass of the body would disappear. There are phenomena in nuclear physics which seem to lend themselves easily to an interpretation of a conversion of matter into energy (such as in the atomic bomb).

A well-known phenomenon of this kind is the so-called "mass defect" of atomic nuclei. The nucleus of the helium atom, e.g., consists of four particles, each of which has the rest mass m_0 of the hydrogen nucleus (proton). However, the mass of the helium nucleus is smaller than four times the rest mass of the hydrogen nucleus. If we assume that the particles in the nucleus are kept together by attracting forces, the particles which are packed in the helium nucleus have a smaller potential energy than they had when separated from one another. The formation of the helium nucleus is connected with a loss of potential energy and therefore with a production of kinetic or radiant energy. If the rest mass of the hydrogen nucleus is m_0 and the rest mass of the helium nucleus $4m_0 - D$, the drop of potential energy accompanying the formation of one helium nucleus is Dc^2. This energy can be regarded as the "binding energy" which keeps the helium nucleus together. There are a great many cases of disintegration of an atomic nucleus where the binding energy can be measured directly. We find a good agreement with the energy calculated from the mass defect.

31. Creation and Annihilation of Mass

Another phenomenon of this type is the creation or annihilation of an electron-positron pair. As we shall learn in Part VIII, the "electron" has a much smaller rest mass than the proton and a negative electric charge.

The "positron" has the same mass as the electron, but the charge is positive. The magnitude of the charges of electron and positron is equal. If they combine, the charges disappear of course. Moreover, there is sufficient evidence to assume that the rest masses disappear also and are converted into radiant energy. This assumption is confirmed particularly by the measurement of the wave-lengths of the radiation which accompanies the disappearance of an electron-positron pair. For (according to sec. 39) radiation consists of elementary portions of radiant energy called photons. The wave-lengths λ of such a photon can be easily calculated from the energy E by the formula $E = hc/\lambda$. Roughly speaking, the smaller the energy, the greater the wave-lengths. If we calculate the loss of energy E produced by the disappearance of the rest mass of an electron-positron pair ($E = 2m_0 c^2$), we obtain the wave-lengths of this radiation from $2m_0 c^2 = hc/\lambda$. It is a very hard gamma radiation and is really found if one observes the phenomena connected with the disappearance of positrons.

We have learned now how the conversion of mass into energy or the annihilation of mass can be described very distinctly and clearly by describing performable physical operations. The annihilation of mass has been interpreted occasionally as a refutation of materialism and support of spiritualism. This can be done only if one uses this language without having in mind the operational mean-

ing of the words and sentences. The statement, "Matter can be annihilated and converted into energy", sounds strange to the man of average school training and smacks at least of spiritualism. For he understands tacitly the words 'matter' and 'energy' the way they are used in everyday language. In this language by 'matter' is meant something like rock or ocean. By 'energy' is meant something like a soul or a spirit. The dualistic view of the contrast between body and mind is deeply entrenched in our everyday language. In this language, 'matter' has the operational meaning of some hard and impenetrable stuff, while 'energy' seems to be defined by operations which belong in the field of psychology rather than in the field of physics.

The physicist, in particular the twentieth-century physicist, means by 'matter' a system consisting of a great number of particles like electrons, protons, positrons, etc. The operational definitions of these particles are very different from the operations by which we used to test the presence of some hard or impenetrable stuff like the "matter" of our everyday language. If we take into account the meaning of the words 'electron' or 'proton', which are used in the wave theory of matter, we are very far from operations testing hardness or impenetrability. On the other hand, radiant energy is regarded by the physicist as consisting of photons. The presence of photons is tested by methods which are essentially of the same kind as the methods by which the presence of

electrons is tested. Therefore, if we introduce the operational meaning of the terms 'matter' and 'energy' and its conversion into energy is not at all obscure and has nothing to do with the experiments of the spiritualists which demonstrate "dematerialization".

VI. Light

32. The Crucial Experiments on Behalf of the Wave Theory

A century ago there were two theories of light both of which were based upon Newtonian mechanics. According to the "corpuscular" hypothesis, light consists of small corpuscles which obey Newton's laws of motion. They perform rectilinear motions with constant speed. If passing from vacuum or air into water, they are attracted by the particles of water and deviated from their original path. The deviating forces are similar to Newton's force of gravitation. From this hypothesis it can be derived that between the angle of incidence a and the angle of refraction β there is the relation $\sin a/\sin \beta = c'/c$, where c is the speed of light in vacuum (or in air) and c' is the speed of light in water. Since by the attraction the light corpuscles are accelerated, it follows that c' is greater than c; the speed of light in water is greater than in air ($c' > c$). But Foucault performed in 1850 an experiment by which he proved that the speed of light in water is smaller than in air in contrast to the claim of the corpuscular theory. Therefore, the second of the conflicting hypotheses about light propagation seemed to be

confirmed: the "wave theory" of light. According to this theory, light consists of waves propagated through an elastic medium (ether) which follows Newton's laws of motion From this hypothesis Huyghens could derive that the speed of light in water is smaller than in air ($c' < c$). This result seemed to be confirmed by the Foucault experiment.

The Foucault experiment has been quoted over and over again as the outstanding example of a crucial experiment. It was supposed to have sealed the death sentence of the corpuscular theory. But if we take a strictly logical viewpoint, we can say only that the experiment decided against a corpuscular theory of light which assumed that the corpuscles follow Newton's laws of motion. Therefore, we can speak only of a decision against any corpuscular theory of light if we mean by 'corpuscle' a small mass in the Newtonian sense of this word. It is obviously not excluded by the Foucault experiment that light consists of corpuscles in a wider sense which move according to laws which differ from Newton's laws. We have to understand that an experiment can only be crucial if only one clear-cut alterna-

tive exists. An experiment can never decide definitely between wave theory and corpuscular theory but at most between Newtonian waves and Newtonian corpuscles. By Foucault's experiment Newtonian corpuscles are outruled, but this does not mean a decision in favor of Newtonian waves, unless we make the assumption that only this alternative exists. Perhaps Newtonian mechanics has to be dropped outright.

33. Conversion of Light into Kinetic Energy; the Photon

Half a century after Foucault's crucial experiment it had become more and more clear that the wave theory of light in its original form did not suffice to cover the whole realm of newly discovered facts. The new facts which have been greatly responsible for the dropping of the full-fledged wave theory of light are the phenomena of "photoelectric effects." If light waves hit the surface of zinc, this surface obtains a positive electric charge. Electrons (negative electric charges) of a mass m and the charge e are emitted by the zinc atoms. The absorbed energy E of the incoming radiation is used partly to overcome the attraction of the surface (work P) and partly to impart to the electron a kinetic energy $mv^2/2$. According to the law of the conservation of energy, this means $E = mv^2/2 + P$ or $mv^2/2 = E - P$.

If the wave theory of light were correct, the intensity of radiation would decrease inversely proportionate to the square of distance from the source of light. If we choose this distance great enough, the energy absorbed by a square inch of our zinc plate tends toward zero. However, experiments show that the kinetic energy $mv^2/2$ of the electrons which originates from the conversion of the incoming light energy into kinetic energy is not dependent at all on the distance of our zinc from the light source. The kinetic energy $mv^2/2$ and the speed v depend only upon the frequency (color) of the incoming radiation.

Einstein interpreted (1905) this result as follows: The light energy is not homogeneously distributed over the wave surface. Therefore, this energy is not thinned out at great distances, as the wave theory implies. On the contrary, according to Einstein's hypothesis, the light energy is concentrated in small packages of energy called "photons". To emit one electron, just one photon of light energy has to be absorbed by the zinc surface. The energy of a photon is proportional to the frequency $E = h\nu$, where h is a universal constant and ν the frequency of radiation. 'Frequency' means "number of vibrations per second". Therefore, the greater the frequency; the larger the photons and the greater the speed of the emitted photoelectric electrons. However, the greater the distance of the zinc from the source of light, the smaller the number of electrons which is emitted per second. But the speed of these electrons depends only upon the frequency of light and not upon the dis-

tance. This hypothesis is formulated mathematically by $E = h\nu$ and therefore $mv^2/2 = h\nu - P$. By checking this equation experimentally, one can determine the constant h. The first elaborate measurement was performed by Millikan. He found the value $h = 6.56 \times 10^{-27}$ erg. sec.

34. The Paths of Photons

If light consists of photons, we must assume that the brilliancy of illumination of a surface is proportional to the number of photons hitting a square inch. If we consider a phenomenon of interference or diffraction, the dark regions on a screen are regions where few photons hit the screen, while the diffraction maxima are regions where a great many photons come in. By this argument one understands easily that the motion of photons does not follow Newton's laws of mechanics but a very different type of law. One has to make use of the superposition of waves to find the distribution of photons. The single photon is not mentioned in these laws at all. The intensity of the light (the square of the wave amplitude) in a certain region is proportional to the number of photons in this region. 'The path of a photon' has no operational meaning. As a matter of fact, this path is not spoken of in any law of physics. We say: Our sun emits photons which heat and illuminate our earth. If we put a screen or a shutter in the way of these photons, we can predict the heat and luminosity effect of the radiation which hits or passes these devices. Since the laws of

wave optics allow us by the superposition of waves to calculate the resultant amplitudes, we know in every region the average number of photons. But the laws of optics do not allow us to describe the path of a single photon on its way from the sun to the earth.

We can also understand more directly why 'the path of a photon' is an expression without operational meaning. If such a path could be physically produced, e.g., a rectilinear path, we could produce a light ray which travels along a geometrical straight line and passes through a definite point in a definite direction. However, the physical existence of such a light ray is incompatible with the laws of wave optics. In order to assure that a particular light ray passes through a definite point in space, we must make it pass through a very small hole in a screen. The smaller the hole, the more precisely the ray passes by a definite point. However, if the diameter of the hole becomes comparable in size with the wave-length of the light ray, the phenomenon of diffraction takes place. The ray passes through a definite point but does not continue in its original direction after passing the hole. If we want to have the ray continuing in its original direction, we must make sure that the hole is much larger than the wave-length. But in this case the beam of light is of a considerable thickness and does not pass precisely through a definite point.

Therefore, if we want to produce a light ray by a physical operation, we

cannot achieve an all-round approximation. Either we (1) bring it about that the ray passes approximately through a given point: then there will be no good approximation of direction; or we (2) achieve a good approximation in direction: then the precise location of the point is poorly defined. This state of affairs has been the basis of the "Principle of Complementarity" in physics.

We must have in mind that a "photon" cannot be thought of as a geometrical point. A package of light energy of a given wave-lengths λ must contain a great number (e.g., N) of crests of waves in order to have the characteristics of a wave with the wave-length λ. Therefore, it must have at least the size $N\lambda$. If the photon is of a size which is small in comparison to $N\lambda$, it cannot have a distinct wave-length or frequency at all. The path of a 'photon' of the frequency $\nu = c/\lambda$ has, strictly speaking, no operational meaning, since only a "point" can traverse a "path".

We are accustomed to speak loosely about a light ray in empty space and to imagine it as a straight line. But here again this way of speaking has an operational meaning only if we assume some physical laws to be true. These laws are, roughly speaking, the independence of the light ray of its environment, in particular of the width of the opening through which it passes. But the validity of these laws means exactly the absence of the phenomena of diffraction, the absence of the wave properties of light. Since

we know that these phenomena exist, 'path of a light ray', without including the environment of the light ray in the description, is an incomplete expression and has no operational meaning.

35. Laws of Motion of a Photon

We can easily give a quantitative description of this state of affairs. We may have a slit of the width A in a dark screen. A beam of light rays comes in perpendicularly to the screen and passes the slit. According to a simple calculation which we find in any elementary textbook of physics, only a fraction of the light energy moves behind the slit perpendicular to the screen. The bulk of it is deviated. The bulk of it is deviated by an angle ψ (the first diffraction maximum) connected with A and the wave-length λ by the equation $A \sin \psi = \lambda$. If there were no diffraction, a light ray passing through a slit which has approximately the shape of a point ($A = 0$) would keep approximately its direction perpendicular to the screen. This means $\psi = 0$ approximately. But, because of the diffraction, A and ψ cannot be simultaneously zero or approximately zero if the wave-length λ is given. To produce an approximately rectilinear light ray, we have to make A and ψ both as small as possible. However, the smaller A becomes, the greater becomes ψ and vice versa. To achieve an approximately rectilinear light ray, we have therefore to compromise and to make both, A and $\sin \psi$, fairly small, e.g., $A = \sin \psi = \sqrt{\lambda}$. If we use light of a wave-

Light

length $\lambda = 10^{-5}$ cm. (ultraviolet), we would obtain $A = 0.003$ cm. and $\psi = 12$ angle minutes approximately. We can describe the light phenomenon by a rectilinear ray if we allow for a margin of 0.003 cm. in the starting-point and of 12 angle minutes in the direction. This means practically a straight line through a point and perpendicular to the screen. The smaller A, the better is the approximation to a rectilinear path which can be made. If we use this "compromise" path instead of the theory of diffraction, we can predict within the margin described where our light ray will hit a screen put up behind the slit.

36. Mechanical Momentum of a Photon

If light radiation hits a material body, it exerts a force upon it—the pressure of light. This can be stated by saying that radiation imparts a certain momentum upon this body. We can interpret this fact by ascribing to every photon a mechanical momentum exactly as to a moving mass. The action of radiation to a body can be treated as collision between masses. Each mass m has a momentum mv, if the speed is v. The sum of the momenta is not changed by the collision, according to Newton's Third Law of motion. If one of the bodies loses momentum, the other one must gain it. A photon has an energy $E = h\nu$. According to the theory of the electromagnetic field, a package of radiant energy E carries a momentum $M = E/c$. This means: If radiant energy E

hits a material body, it imparts the same momentum as if a mass m with the speed v hit the body, mv being determined by $mv = E/c$. A photon in particular has the momentum $M = h\nu/c$. Since every photon travels with the speed of light c, this momentum does not depend on speed but on the frequency or wave-length. Since $\nu = c/\lambda$, we have

$$M = \frac{h\nu}{c} = \frac{h}{\lambda}.$$

This momentum can be directly checked by the Compton effect, which we observe when we examine the collision of a photon with an electron. By this collision the momentum of the electron as well as that of the photon is affected. The alteration of the momentum of the photon means an alteration of the wave-length. If the electron has originally the momentum of zero, the momentum of the photon can only decrease by the collision. This means that the wave-length λ must increase and that the frequency ν must decrease. From the hypothesis that the momentum of the photon is $M = h/\lambda$, one can derive that the wave-length of a photon which is moving after the collision at a right angle to its original direction increases by h/mc, where m is the mass of the electron. $h/mc = 2.42 \times 10^{-10}$ cm. is called the 'Compton wave-length'. This alteration of wave-length can, of course, be observed only if the original wave-length is very small. Compton carried out this experiment by using X-rays the photons of which have very short wave-lengths. He could

confirm the alteration of the wavelengths of X-rays which hit some material bodies which are rich in electrons. This experiment is the most direct measurement of the mechanical momentum h/λ of the photon.

37. The Interpretation of Diffraction in Terms of the Mechanics of the Photon

If we keep in mind that the photon carries a mechanical momentum, we can give to the diffraction experiment also a purely mechanical interpretation. Instead of attempting to produce an exactly rectilinear light ray, we can attempt to produce a photon which has a certain definite position and a momentum in a definite direction. We determine the position again by having the photon pass a slit of a size A in a screen. If no diffraction existed, the whole momentum would be perpendicular to the screen; the momentum parallel to the screen would be zero. By decreasing A more and more, we could achieve with an arbitrary accuracy that the photon passes through a given point and has a momentum exactly perpendicular to the screen. But, because of the diffraction, the momentum of the photon has also a component parallel to the screen. Since the momentum itself has the magnitude h/λ, its component parallel to the screen is $h/\lambda \sin \psi$. We denote this component by 'p'. From $A \sin \psi = \lambda$ (sec. 35) follows $A \sin \psi = h/\nu$ $\sin \psi$ or $Ap = h$. We see again that it is impossible to produce a photon which has simultaneously an exact

position and an exact direction of momentum (perpendicular to the screen). For in this ideal case we would have $A = 0$ and $p = 0$, which would render $Ap = h = 0$, which is impossible, since h is different from 0. To achieve an exact position ($A = 0$), we use an infinitely small slit. In this case p would increase infinitely. We could not say anything about the direction of the momentum. If we want an exact direction of momentum (p very small), we need a very great A. This means that the position of the photon becomes undefined. We can again compromise by making A and p both fairly small. Then we have a photon with a fairly exact position and fairly exact direction of momentum.

We understand very clearly by these considerations that the photon is not a particle with a certain position and momentum which both exist but cannot be measured simultaneously. Actually, an experimental arrangement which produces a photon with an exact position (very small A) frustrates the achievement of an exactly perpendicular momentum (p very small). 'A photon with an exact position and an exact momentum' is an expression which does not enter into any description of physical facts; it does not enter into the formulation of any physical law. Therefore, this expression has no operational meaning and has to be dropped from the vocabulary of physics.

If we assume the x-axis parallel to the screen, we may introduce the

symbols $A = \Delta x$, $p = \Delta p_x$, where Δx means the inexactitude of the position and Δp_x the inexactitude of the momentum, both in the x-direction. If we describe a state of affairs by saying that our photon has a position $x = 0$ and the momentum $p_x = 0$ parallel to the screen, this description is only correct if we allow the "uncertainty" Δx in the position and Δp_x in the momentum. Since $Ap = h$, we

have the relation $\Delta x \times \Delta p_x = h$ between these "uncertainties".

If we use the description $x = 0$, $p_x = 0$ for the present state of the photon, we can predict the future with the "indeterminacy" Δx and Δp_x by assuming a rectilinear path perpendicular to the screen. These remarks are the roots of W. Heisenberg's relation of "uncertainty" or "indeterminacy" (sec. 43).

VII. Mechanics of Small Masses (Wave Mechanics)

38. Failure of Newton's Mechanics in the Domain of Very Small Particles

Newton's physics assumed that the laws which govern the motion of the planets govern also the motions of the smallest particles of matter—the atoms and electrons. One concluded that the orbits of the electrons revolving around the atomic nucleus would follow the same law as the orbits of the planets around the sun, except that the gravitational forces in the solar system had to be replaced by electrostatic attractions in the atom. However, both types of forces followed one and the same law: The force is inversely proportional to the square of the distance. But in quite a few cases, and in crucial ones at that, the Newtonian pattern failed to render an adequate method of describing and predicting the conditions of atomic equilibrium and motion.

In spite of the general belief that the

action of chemical valences can be described by the Newtonian concept of force, it has been actually impossible to substitute into the Newtonian pattern '$ma = f$' an expression for 'f' which would describe the phenomenon of saturation of a valence. It is easy to understand that two particles of unlike electric charges (positive and negative ions) can attract each other. But how can two neutral particles form a compound? However, just this seems to happen in the case of very simple chemical compounds (homopolar compounds). The hydrogen molecule H_2 consists of two neutral hydrogen atoms. The chemist says that the valences of these atoms saturate each other. But from the point of view of the mechanics of electrostatic forces it is hard to understand how this can happen and how a third hydrogen atom can be prevented from being attracted. The phenomenon of saturation of valences remains unexplained in Newton's mechanics.

Still more obvious is the failure of Newton's pattern to describe the motion of electrons in the atoms which must occur in order to explain the emission of the spectral lines. According to the theory of Niels Bohr, the emission of spectral lines from heated hydrogen gas is a kind of reversed photoelectric effect. If a photon hits the surface of a metal, the radiant energy of the photon is converted into the kinetic energy of an electron leaving the surface (sec. 33). Similarly, if the energy (kinetic and potential) of the moving electrons within the atom decreases, the mechanical energy lost by the atom is converted into the radiant energy of the photons which leave the atom. These photons constitute light of a specific wave-length which reveals itself by the spectral lines of the hydrogen gas.

39. Bohr's Theory of the Emission of Light by the Atom

When the atom passes from the energy E_n to the smaller energy E_1, according to Bohr's hypothesis exactly one photon of the energy $h\nu$ is produced. If we accept Einstein's fundamental hypothesis about the conversion of mechanical energy into radiant energy and vice versa, we have the equation $E_n - E_1 = h\nu$. If we know the initial and final energy of the atom (E_1 and E_n), the frequency ν of the emitted radiation is determined. We know from the examination of the frequencies of spectral lines that there are specific frequencies in the hydrogen spectrum. Therefore, there can be

only specific values of energy in the hydrogen atom.

If we consider the simplest case, we can assume that the negatively charged electron is moving along a circle around the positively charged nucleus. According to Newton's laws of motion and Coulomb's law of electrostatic forces, the attraction e^2/r^2 must be balanced by the centrifugal force mv^2/r, where e is the charge of the electron and of the nucleus, m the mass of the electron, v the linear speed, and r the radius of the circular orbit. From $e^2/r^2 = mv^2/r$ it follows that $v^2r = e^2/m$. This means that for every radius r a circular motion is possible if we choose only the speed v accordingly. The value of the energy E of our circular orbit is given by

$$E = - e^2/r \text{ (potential energy)}$$
$$+ m v^2/2 \text{ (kinetic energy)} = - e^2/2 r .$$

Since there is an orbit for any value of r, there is also an orbit for any value of energy E. Therefore, Newton's mechanics cannot help us to derive the specific values of energy (E_n and E_1) which render specific values of frequency. Bohr, following some suggestions of Planck's theory of radiation, assumed that the orbits of electrons have to obey, besides Newton's laws of motion, a still different kind of law, called "quantum laws," by which most of the orbits allowed by Newton's mechanics are excluded.

These quantum laws require that the angular momentum of the revolving electron has to be equal to $h/2\pi$ (where h is the same constant as

in secs. 33–37) or an integer multiple of $h/2\pi$. This means that mrv is equal to $h/2\pi$ or $nh/2\pi$, where n is an integer number. Then it follows that the smallest possible value of the radius r can be derived from $2\pi mrv = h$ and $mv^2r = e^2$. By eliminating v, it follows that $4\pi^2m^2r^2v^2 = h^2$, $4v^2mr = h^2/e^2$, and, finally, $r = h^2/4\pi^2me^2$. This value of r provides us with the smallest possible size of an electronic orbit. The charge e of the electron is known from Millikan's experiment on the ionization of oil droplets; m is known from J. J. Thomson's experiments on the deflection of cathode rays in magnetic and electric fields; h is known from the photoelectric effect (sec. 33). From the known values of h, m, and e we obtain that r is approximately 10^{-8} cm. This value agrees with the size of the atoms obtained by other methods, e.g., Brownian motion, diffraction of X-rays by crystal lattices, etc.

Knowing r, we can calculate the energy E_1 of the smallest orbit ($n = 1$). We obtain $E_1 = -2\pi^2e^4m/h^2$. To obtain the energy of the nth orbit, we have to replace h by nh and obtain $E_n = -2\pi^2e^4m/h^2n^2$. This formula allows us, by using the Einstein-Bohr formula $E_n - E_1 = h\nu$, to calculate the frequencies of the spectral lines of the hydrogen atom in agreement with the experiments.

40. The Wave Theory of Matter

This was a great success of Bohr's spectral theory, but we must not forget that it was achieved by a partial abandonment of Newton's mechanics without replacing it by a new mechanics. The crucial formula, $4\pi^2m^2r^2v^2 = h^2$, was obtained by a hypothesis (quantum law) which was superimposed upon Newton's laws of motion, leaving these laws themselves substantially unaltered. In this way mechanics and optics became an incoherent patchwork which gave rise to many pseudo-problems.

The French physicist, Louis de Broglie, looked at this situation from a new point of view. He did not believe in patching up Newton's laws by the quantum laws but in altering Newton's laws themselves. He took his cue from a comparison between optics and mechanics. The optical phenomena can be described and presented in terms of light rays (straight lines in vacuum) in every case where diffraction can be neglected. If diffraction comes in, we have to pass from ray optics (geometrical optics) to wave optics. Under what conditions must diffraction be considered? Obviously, diffraction comes into the picture if light passes through small openings or around small obstacles, the word 'small' meaning 'comparable in size with the wave-lengths of light'. This means that the pure ray optics loses its applicability if the rays have a great curvature. The word 'great' means here again a radius of curvature which is comparable with the wave-lengths of light. Perhaps, argued De Broglie, the orbits calculated from Newton's mechanics play only the role of the light rays in optics. Perhaps Newton's mechanics loses its applicability also

if the orbits have a very large curvature or a very small radius of curvature. This is probably the case if we consider the curvature of the electronic orbits around the nucleus of the hydrogen atom. The radius of curvature of the smallest orbit is of the order of magnitude of 10^{-8} cm., much smaller than the wave-lengths of visible light, which is about 10^{-5} cm.

Under such circumstances, De Broglie argued, we have to replace Newton's mechanics by a new mechanics. This generalized mechanics would be in such a relation to Newton's mechanics as wave optics to ray optics. For small curvature we can regard the optical rays as paths of the photon. But, if diffraction comes in, the behavior of photons can no longer be described by paths (secs. 34, 35).

Louis de Broglie advanced the idea that there is a new type of waves called by him "waves of matter" and later, after him, "Broglie waves" which determine the behavior of material particles and electrons in a way similar to the way in which the light waves determine the behavior of photons. The most natural hypothesis was to assume that the relation between wave-length and mechanical momentum is for the material particle the same as for the photon. We know that for photons the momentum p obeys the formula $p = h/\lambda$. If we know the λ of the kind of light used, we can calculate from this formula the momentum p. In the case of material particles, however, the momentum is known from mechanics $p = mv$ (v be-

ing the speed of the particle), while the wave-lengths of the De Broglie waves must be calculated from the formula $\lambda = h/p = h/mv$.

41. The New Laws of Motion for Small Particles

One notices immediately that if the mass m of a particle tends toward infinity, the Broglie wave-lengths λ tend toward zero. This means that the phenomena of diffraction can be neglected. The motion of great masses can be treated without using the waves. We have to do with orbits of masses which obey Newton's laws of motion. But if we have to do with very small particles, the Broglie wave-lengths become comparable with the radius of curvature of the orbit, as in the case of the electronic orbit in the hydrogen atom, and we have to apply the laws of "wave mechanics". According to these laws the term 'path of a particle' has no more operational meaning than 'path of a photon' in ordinary optics. What we describe according to Newtonian mechanics as a stream of electrons emitted by some source (heated wire or the electrodes of a vacuum tube) must now be described as the emission of "Broglie waves". Exactly as the intensity of the electromagnetic waves describes the number of photons per unit of volume in a certain region, the intensity of the De Broglie waves describes the number of material particles per unit of volume—the density of mass.

The first confirmation of this daring hypothesis was De Broglie's deriva-

tion of Bohr's "atomic radius" without using the quantum laws. The wave propagation along the circular orbit of the electron around the nucleus must not destroy itself by superposition and interference. Therefore, the perimeter of the smallest orbit must be equal to one wave-length. This means, according to De Broglie, $2\pi r = \lambda = h/mv$. This result is obviously equivalent to Bohr's quantum law $mrv = h/2\pi$ (in sec. 39).

42. Diffraction of Material Particles

The most direct confirmation of De Broglie's hypothesis, however, would be to show that material particles (e.g., electrons) which pass a small hole produce on a screen a diffraction pattern similar to the diffraction pattern of light. There would be alternatively dark and bright rings. The bright rings are the regions of the screen where a great many particles hit, while the dark rings show us the regions where no, or very few, particles hit. The size of the hole must be comparable to the wave-lengths of the De Broglie waves. The American physicists Davison and Germer used the intervals between the atoms of a metallic foil as slits to produce a diffraction pattern by having electrons pass this foil.

By these and similar experiments it has become evident that very small material particles follow laws of motion which are very different from the laws of Newtonian mechanics. The particles of a beam which pass through the hole in a direction perpendicular to

the screen do not continue moving in this direction behind the screen as they are expected to according to Newton's law of inertia. Only a certain percentage of the incoming particles do so. They hit the center of the receiving screen and form the central maximum of diffraction. The greatest percentage of the remaining particles is diffracted toward the first diffraction maximum by the diffraction angle ψ, where $A \sin \psi = \lambda$. If we know that, e.g., a thousand particles enter the hole, we cannot predict what every individual particle will do behind the slit. But we can safely predict that a certain percentage will hit the central maximum and a certain percentage the first diffraction maximum ψ, etc. Here again, as in the case of the photon, we can denote the size of the slit by Δx (this means $A = \Delta x$) and the momentum of the particle parallel to the screen by Δp_x. Since the formulas are the same as in the case of the photon, we have again $\Delta x \times \Delta p_x = h$. The product: the "uncertainty of x" times the "uncertainty of p_x" equals a constant h.

43. The Uncertainty Relation

The meaning of this relation in the case of material particles seems to be much more paradoxical than in the case of the photon. For this relation is incompatible with the basic statement of Newtonian mechanics that every mass point has at every moment of time a definite coordinate x and a definite momentum $p_x = mv_x$ in the direction of x. If we know these values

for an instant of time $t = 0$, Newton's laws of motion allow us to predict these values for any future instant of time. If we examine carefully our result obtained in the case of photons and apply it to the case of small material particles, we learn that there is no law of mechanics which contains the expression 'position and momentum of a particle at a certain instant of time'. There are experimental arrangements which we can describe by saying that the particle passes the diaphragm at a position $x = 0$ with a margin of error Δx; by narrowing the slit, we can make this margin Δx as small as we wish. But then the equation $\Delta x \Delta p_x = h$ shows us that Δp_x becomes very great. This means that we only know that behind the slit the momentum p_x of the particle is zero with the allowance of a wide margin of error. If we consider a great number of particles passing the slit of the width Δx, most of them have a momentum $p_x = h/\Delta x$ parallel to the screen. But also all smaller and greater momenta will occur with a certain frequency according to the Gaussian distribution of errors. If we fix the position of the diaphragm and of the small hole at $x = 0$ relative to the earth, we obtain diffraction rings on a screen which is at rest behind the slit. We can predict the position of these rings exactly from knowing only the position of the screen relative to the slit. Our information which allows us to predict the behavior of the particles behind the slit is based entirely upon the measurements (in the ordinary sense of the word) which we

can perform in the experimental setup. In this way we know $A = \Delta x$ (width of the slit) and the distance of the screen from the slit. This information allows us to predict the position of the diffraction maxima on the screen. The momentum Δp_x of an individual electron while passing the slit remains unpredictable. But obviously it is possible to "measure" the momentum of an individual electron in a certain sense. Since the component Δp_x of this momentum is directed parallel to the diaphragm, the particle will, in passing the slit, impart a momentum upon the diaphragm parallel to the diaphragm itself. If the diaphragm is not fixed with respect to the earth, but movable, one can find the momentum of the particle by measuring the momentum of the diaphragm, which means practically its speed obtained by the impact of the particle. But if the screen is moving while the electrons pass the slit, the position of the particle relative to the earth is not predictable even if the slit Δx is very small.

We must always decide in what result we are interested: in the position of the particle passing the diaphragm, or in the momentum of this particle. In each case we can make a prediction. In the first case we can predict the diffraction pattern on the screen. In the second case we know the momentum which the diaphragm gets from the particle, and we can make predictions by means of Newton's mechanics. We can predict the motion of bodies which are hit by the

diaphragm. We could, e.g., predict the elongation of a ballistic pendulum.

44. Misunderstandings about the Relation of Uncertainty

We must carefully avoid the misunderstanding which has been caused by the way some physicists have discussed the relation of uncertainty. One hears sometimes the statement: "It is impossible to measure simultaneously the position and the momentum of a small particle". This sounds as if there would be small particles which possess certain positions and certain momenta. We are told that we can measure either of them but that nature is so diabolic as to prevent us from measuring both simultaneously. This statement is rather misleading. The expression 'a particle with a certain position and a certain momentum' has no operational meaning if De Broglie's hypothesis is accepted. However, we can set up an experimental arrangement which gives to the expression 'particle with a certain position' a kind of approximate operational meaning. We can make some predictions from such statements. They determine the future within a certain margin of "indeterminacy". There are also arrangements which allow us the use of the expression 'particles with a certain momentum'. They lead to predictions which are also reliable within a certain margin. The relation of "uncertainty" involves the relation between these two margins of prediction and is called, for this reason, the "relation of indeterminacy". But "a particle with

a certain position and a certain momentum" is not an object of the physical reality as far as very small particles are concerned. Speaking exactly, a particle by itself without the description of the whole experimental setup is not a physical reality. But "a particle passing a diaphragm at a certain point" is a description of a physical reality and "a particle imparting to a diaphragm a certain momentum" is a description of a physical reality too.

As a matter of fact, we would find all these statements less paradoxical if we dropped the word 'particle' altogether from the language of physics. We could say that the mechanics of the smallest particles, wave mechanics, allows us from the knowledge of the initial experimental conditions to predict the future observable phenomena. Among these phenomena are in particular "point events", e.g., scintillation produced at a point of a screen, and "impulse events", e.g., a momentum imparted to an observable material body. Then nobody would be puzzled by learning that scintillations on a screen do not follow an orbit which is determined by Newton's laws of motion. Hans Reichenbach pointed out that small particles with definite positions and definite momenta can be introduced into the language of physics and that their introduction can be justified in a certain sense. The logical coherence of the system of physics would not be violated, but the motions of these particles would follow very awkward laws. Reichenbach speaks of

"causal anomalies". He calls this way of describing the subatomic phenomena the "exhaustive interpretation", while he calls the presentation in Bohr's complementary language as in the present monograph the "restrictive interpretation". The latter sticks strictly to experience and to simple operational definitions but sacrifices the traditional language of mechanics. The exhaustive description sticks strictly to the language of Newtonian mechanics but requires very involved operational definitions. It gives a certain satisfaction to the lover of logic but does not give the simplest possible description of the subatomic phenomena.

45. Bohr's Idea of Complementarity

As Niels Bohr has pointed out repeatedly, the physicist feels at ease when he can keep to the language of the mechanics of everyday life as far as possible. However, if we stick to "simple and practical" operational definitions there is no adequate means of describing the subatomic phenomena in terms of "full-fledged particles" with position and momentum. In this situation the physicist takes some satisfaction in speaking, at least, of "particles which have only position" and of "particles which have only momentum". In this way we use, at least partially, the language of corpuscles to which we are accustomed in Newton's world.

If we admit any operational definition without regard to whether it is "simple and practical", we can, of course, measure the exact position and velocity of a particle and give an "exhaustive interpretation" of its state. We use two diaphragms with small slits and observe a particle which passes both slits. Then the velocity can be calculated from the distance of the slits. But the velocity calculated in this way is not the velocity which helps us to predict the future course of the particle. For after the passing of the second diaphragm we must again take into account diffraction, and the future course cannot at all be calculated from the velocity. The "exhaustive interpretation" of the state of a particle is no basis for predicting its future states by simple laws. Therefore, the correct statement of the "relation of uncertainty" is not that "position and velocity cannot be measured simultaneously" but that "there is no law of prediction which contains reference to the simultaneous position and velocity of a particle". If a statement does not contain a rule of prediction, it is not a physical law but purely tautological. It is a definition. Our statement about measuring velocity by using two diaphragms is certainly an operational definition of position and velocity. But this definition does not enter into any law of physics which would help us to predict the future course of events. As we emphasized in section 2, an operational definition is only "helpful" or "practical" if it helps us to formulate physical laws.

The position and the momentum of a particle are two physical properties

which reveal themselves on different occasions which exclude each other. The position can be obviously observed as a "point event" and the momentum as an "impulse event"—they have nothing to do with each other. Only if the particle has such a great mass that Newton's mechanics can be applied, can we speak of a particle having a position and momentum and can we calculate the momentum from two positions by the formula $p = mdx/dt$. According to Bohr, the physical world cannot be described by one coherent language. There are two languages which "complement" each other. Under certain circumstances the language of "positions of particles" or "point events" must be used; in other circumstances, excluding the ones above mentioned, we speak of "momenta of particles" or "impulse events". If we make use of all possible information about the present state of the world, we must use both languages. Then we can predict all events which our actual science enables us to predict. This aspect of the world is what Bohr has called the "aspect of complementarity".[25]

The indeterminacy relation $\Delta x \Delta p_x = h$ can also be written $\Delta x \cdot \Delta v_x = h/m$ if we introduce $p = mv_x$. If the mass m increases, h/m tends toward zero. It becomes more and more meaningful to assume that Δx as well as Δv_x are 0 and to speak of a mass with a definite x and a definite v_x.

We understand now very well what prediction can be made if we send particles through a very small slit in a diaphragm. We consider, e.g., a beam of electrons directed perpendicularly to the diaphragm. We proceed as we did in optics (sec. 35) in order to obtain a "compromise beam". This means to make the size of the slit rather small but not so small as to make the momentum undefined. Then we obtain what we call in experimental physics a "beam". According to Newton's mechanics, it would follow from the law of inertia that all electrons, after passing the slit, continue in their course perpendicular to the diaphragm. They strike a screen which is set up parallel to our diaphragm within a region which is nearly congruent to the slit. However, if diffraction plays a certain role, there is for every region of the screen a certain possibility of being hit by a particle. Instead of the rigid law that one limited region is hit and all the rest of the screen remains untouched, we have to say that the frequency of a hit is distributed over the whole screen. One says sometimes, and it means the same thing, that the law of inertia is replaced by a law predicting the statistical distribution of hits. We can only say that in the region where, according to Newton, all particles should hit there is merely an overwhelmingly great frequency of hits.

46. By What Law Does Wave Mechanics Replace Newton's Second Law?

We assume now that the beam of electrons passes a "field of force". We consider only the simplest case where

the force has the direction of velocity. The force f is defined by the formula $f = ma$, where 'a' means the acceleration of a "great mass" m to which Newton's law can be applied. We assume, e.g., that this beam is launched upward against the force of gravity. The acceleration of gravity at this point of the earth may be g. The particles may start with an initial speed v. According to classical mechanics, they would move upward by a distance s which is given by the equation $mv^2/2 = mgs$ or $s = v^2/2g$ (decrease of kinetic energy is equal to the work done against the force of gravity). If the mass m is "small", Newton's mechanics can no longer be applied. By "small" we mean that there is a considerable drop of potential between two points with a distance of one "Broglie wave length": $\lambda = h/mv$. We find now from the laws of wave mechanics that we can affirm only that most of the particles reach the height of $s = v^2/2g$. But there is for every height s a certain percentage of particles which reach it. Newton's law of motion ($ma = f$, where f is the force of gravity) is replaced by a formula which gives us the percentage of particles which reach a certain height s.

This reshaped law of motion becomes particularly important if we replace the force of gravity mg by an electric field which acts upon the charge of the electron. If the potential difference between the diaphragm and the final position of the electron is V (voltage), the force in the Newtonian sense is eV/s. If the mass m is so great

that Newton's mechanics can be applied, the distance s which an electron traverses is again given by $mv^2/2 = eVs$, and therefore $s = v^2m/2Ve$. If the mass is "small", we can no longer predict that all particles will reach s but say only that most of the particles reach the distance indicated by the last formula. However, some particles go farther. There is for every distance s a certain percentage of electrons which reach it.

This result of the new mechanics turned out to be particularly important for the understanding of the phenomena of radioactivity. The alpha radiation of radioactive substances (like radium) consists in the emission of positively charged particles (alpha particles) from the nucleus of the atom. However, if we measure the speed v of the emitted particles, we find that, according to Newton's laws of mechanics, they must have traversed a considerable electrical potential difference in order to acquire this speed. But in this case the nucleus must be surrounded by a very high "potential barrier". This means that there is also a considerable potential difference between the center of the nucleus and its surface. But then it can be calculated from Newton's laws and the observed speed v that the charged alpha particles will not succeed in "ascending" this potential difference and in leaving the atom, as our v would be too small. The Russian physicist Gamow pointed out that, according to wave mechanics there is for any initial value of v a certain percentage of particles

which will succeed in reaching the surface of the nucleus and be emitted into the open. In this way the new mechanics finds a wide field of application in the motion of the subatomic particles. According to the new mechanics of small particles, we can start from the observable initial conditions, e.g., the initial electron beam, and predict future observable phenomena. We are not able to predict every single point event, but, if the "force" is given, we are able to predict the statistical distribution of the future point events.[25]

47. Physical Reality and Causality

One must not exaggerate the gap between the new mechanics of small masses and the Newtonian mechanics. According to Newton's mechanics, the future positions and velocities of mass points can be predicted with certainty. This is, of course, only true if we speak in the language of the "calculus" (sec. 1), the language of "physical quantities". However, if we substitute for these quantities the operational meaning, some statistical component always enters. Speaking in terms of sense data, the distinction between prediction of a single event and statistical prediction loses even its clear meaning. For the question always remains arbitrary to a certain degree of what we are to regard as a single sense datum and what as an average of a great many sense data. The difference between a statistical theory and a strictly causal theory is not as much in the realm of physical quantities.

Newton's mechanics allows a strictly causal prediction of the positions and velocities of mass points which are regarded as the physical reality described by this theory. But what is the physical reality described by the new mechanics? If we regard the percentage of particles producing point events in a certain region (the wave function) as this reality, the new theory is causal too. It only loses its causal character if we regard the positions and momenta of particles as the state variables which describe the physical reality. But these variables are in the new mechanics not state variables in the sense in which this word is used in Newtonian physics. There is no description of the world at a certain instant of time which contains both position and momentum of a mass point.

Moreover, while in Newton's mechanics the state of the mass point is described without an experimental setup to which it belongs such a "mass point stripped of its environment" does not occur at all in the new description of the world. This new description which contains, e.g., the term 'position of a mass point' gives a description of the present state of the world by describing actually objects like "a mass point in a particular experimental set-up." These objects have, obviously, operational meaning. And there is a "complementary" description of a second setup which allows us to ascribe a "momentum" to the particles. But there is no setup which would allow us to use both the terms 'position' and

'momentum of a particle'. Obviously, we can make only predictions, starting from statements which have operational meaning. Therefore, we can make predictions starting from a statement about the "position of a particle". But these predictions are not so precise as the predictions in Newton's physics which start from statements about both "position and momentum of a particle". To say, therefore, that in wave mechanics the law of causality is not as strictly valid as in Newton's mechanics would be a very inadequate description of the character of the new mechanics. For these two systems of mechanics do not use the same state variables to describe the physical reality. Therefore, we cannot compare them as to the greater or smaller validity of the law of causality. Such a comparison would make sense only if the old and the new description made use of one and the same set of state variables. But this could be achieved only by reducing both descriptions to the language of sense data or to the "thing-language". But in this language the distinction between a "causal" and a "statistical" theory becomes vague.

The new mechanics, we are often told, does not describe physical reality at all. For the state of a photon or of a mass particle is never described objectively but always in connection with an experimental setup or an instrument of measurement with which it is in interaction. We cannot, e.g., describe the state of a photon on its way from the sun. We can describe it

only at the instant of time when it strikes a screen or comes otherwise in interaction with a material body which has so great a mass that its behavior can be described by Newtonian mechanics. Even a revolutionary mind like Einstein's has for a long time doubted whether it is advisable to drop the traditional conception of objective physical reality, according to which "physical reality" is ascribed to a material particle without regard to its environment. Einstein has suggested not to stop efforts to find a theory of subatomic phenomena which describes "real things" in the traditional sense. Obviously, there is no convincing reason which could prevent us from sticking to the Newtonian concept of physical reality and from regarding Bohr's "Principle of Complementarity" as a provisional stage of science. But one may as well admit a different type of world description in which the expression 'position of a particle' cannot be applied without describing the instrument by which this position is measured.[27] If this description is "simple and practical", we can as well ascribe "physical reality" to the objects of our new mechanics, provided we mean "reality" in the operational and not the metaphysical sense.

If we examine the whole question of causality more thoroughly, we soon notice that this law in its whole generality cannot be stated exactly if the state variables with which the world is described are not mentioned specifically. Otherwise, the general formulation of the law of causality would have

no operational meaning. For the more general formulation says: let a state A of the world be succeeded by a second state B. If A occurs again, B follows again too. If no specific state variables are used for the description, we cannot check whether the state A has recurred. Moreover, only if we know that certain physical laws are valid can an operational meaning be assigned to the state variables (sec. 3). Therefore, the type of physical laws which are to be set up is dependent upon the kind of state variables which are used.

48. Object and Subject in Wave Mechanics

Frequently we are told that in the new physics the role of the perceiving subject is greater than in Newtonian mechanics. The observing physicist has to be introduced explicitly into physics, while previously physics has dealt only with the observed object.

This assertion must be taken with a grain of salt. We have seen that in wave mechanics the state of a mass point can never be described without including the instrument of measurement which is used. We must state explicitly whether we use a narrow slit or a wide slit in a diaphragm in order to describe the position of a mass point. If we call the mass point the physical object which we wish to describe, we must include into the description the instrument of measurement. This instrument is a medium-sized mass and can be described by using the language of Newton's mechanics. But there is

no sense in calling this instrument of measurement the "observing subject."

If we use the expression "observing subject" as it is used in the psychology of everyday life, we have to say that the observing subject (the living physicist) observes the instrument of measurement in the same way as he observes any object of Newtonian physics. Therefore, by "observed objects" we have to understand in the new mechanics, as well as in Newton's mechanics, the medium-sized mass, the instrument of measurement, the scale or balance which one observes immediately. The electron which passes a diaphragm must not be called the "observed object" if we want to avoid ambiguities. The "electron" is a set of physical quantities which we introduce to state a system of principles from which we can logically derive the pointer readings on the instruments of measurement. If we call these instruments "physical objects", the "electron" or "photon" can be called so only by a certain shift in the sense of the term 'physical object'. But we must never forget that there is no meaning in the question of what is "really" a "physical object". The only problem is to agree about an unambiguous use of this word. The loose way of speaking which is so customary in the borderland between physics and psychology has even succeeded in confusing the instrument of measurement with the "perceiving subject".

To say that the observer destroys by the very act of observation some

property of the object which he wants to observe is a misleading formulation too. The observer has nothing to do at all in this matter. His role is exactly the same as in Newtonian physics. The "momentum of a particle" cannot be destroyed by observing the position because this momentum has never existed except in so far as we have a setup which allows the definition of a "momentum". All this sensational "destruction by observation" is an oversimplification. We found a similar situation in the theory of relativity. The introduction of the observing subject was misleading too. The correct thing to say is that, by setting up an experimental arrangement which would allow us to define 'the position of a particle', we do not "destroy the momentum of this particle". What is "destroyed" is only the possibility of setting up the "complementary" arrangement, which would allow us to define the term 'momentum'. The role of the living observer is also in "Relativity" only to observe "clocks" or "yardsticks" which can be described in the language of everyday life.

49. Metaphysical Interpretations of Wave Mechanics

Quite a few authors have maintained that by the new mechanics an "irrational element is introduced into physics" or that "physics is now supporting an idealistic world picture" or that "physics is now in agreement with the doctrine of free will" or even that "by the wave mechanics for the first time in the history of human thought the conflict between religion and science has been settled." If we pursue precisely the analysis of the logical structure of the new mechanics, we will understand that there is no foundation for all these philosophical interpretations of the new mechanics.

The basis of these interpretations has been provided by some metaphysical formulations of the new mechanics which have been given by a great many philosophers and, for that matter, by quite a few physicists and mathematicians. Such a formulation is, e.g., the introduction of the observing subject as a mental, immaterial "entity" into physics itself. This way of speaking, as we have already seen, is inadequate. By others the new mechanics is described as introducing a physical object which is both particle and wave. Some authors speak of it as "particle and/or wave" and some authors have even coined new words like "wavicle" to denote this hybrid object. As a matter of fact, such an object resembling a centaur, who was half-man and half-horse, does not exist in the new mechanics. There are experimental setups which can be described by using the term 'position of a particle' and others which can be described by using the terms 'momentum' or 'wave-lengths'. All the confusion is produced by speaking of an object instead of the way in which some words are used. We have again an example of what Carnap calls the fallacy of using the "material mode" of speaking instead of the "formal

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mode". In a great many presentations of the new mechanics it is not even pointed out clearly that the so-called "dualism between particle and wave" is only another expression for the complementary use of the words 'position' and 'momentum of a particle'. As in a great many other cases, an ambiguous way of speaking is introduced by the predilection of quite a few authors for an "ontological" way of speaking. Some scientists who are very competent in their fields succumb to the temptation to make statements on ontology. This is quite natural since ontology is nothing but the use of our everyday language in a domain where it loses its meaning.

The mental or idealistic character of the new mechanics is occasionally demonstrated by calling the De Broglie waves "waves of probability". Since "probability" is often used as a psychological concept, we describe in wave mechanics the physical world by using terms of psychology. This interpretation is certainly a misleading one. The new mechanics describes the percentage of electrons which strike on the average a certain region of the screen. There is nothing psychological involved, or at least there is no more psychology than in any branch of physics. For even in Newtonian physics every statement is accompanied by its operational definition. And this definition contains the description of physical instruments. This description again makes use of words which express sense data like 'blue', 'warm', 'soft', etc. These words are the only

psychological element in modern as well as in traditional physics.

Another way in which metaphysical misinterpretation enters into physics is the use which is made of the word 'real' in contrast to the word 'fictitious'. Quite a few authors would say that the De Broglie waves are "not real waves" but only a "fiction" which serves to describe the path of particles. This way of speaking is also misleading and is not in agreement with a coherent presentation of physics. We have learned that the movement of photons is determined by the electromagnetic waves exactly in the same way as a movement of small particles (electrons) is determined by the De Broglie waves. The reality of the electromagnetic waves can be demonstrated only by checking the effect of the photons on the bodies with which the waves come in contact. In the same way the reality of the De Broglie waves can be demonstrated too.

Quite a few philosophers and scientists have claimed that the "Indeterminacy Relation" gives a support to the philosophical doctrine of "Free Will". However, no statement can be supported by physical theories which cannot be formulated in terms of physical operations (sec. 2). The statement "the will is free" has, certainly, no operational meaning. It is a purely metaphysical statement and cannot be supported by any physical theory (secs. 2 and 4). Therefore, all the talk about the intrusion of mental and psychological elements into physics has its source only in an inadequate presentation of the recent parts of physics.

VIII. Structure of Matter

50. Continuity or Discontinuity of Matter

It has been an age-old dispute whether matter tightly fills the world space or whether matter consists of small indivisible particles, "atoms", between which there is empty space. The existence of these atoms has been suggested by many facts, in particular the law of constant proportions in the chemical compounds, the elastic properties of gases, etc. However, it has been held out against the old atomistic theories that nothing can be won by assuming that a big piece of iron consists of very small pieces of iron, called atoms. For then the question has to be raised: What is the structure of the small piece of iron called atom? If it is iron, it must have the same structure as the big piece, and so on.

The physics of the twentieth century has come to the firm conviction that the mass density inside solid bodies shows conspicuous maxima and minima which have specific distances from each other. These distances are characteristics of the physical and chemical constitution of the body. They are usually interpreted as the intervals between the centers of atoms. But it would not be correct to say that in these intervals between the atoms the space is "empty" and that the density of matter has the exact value zero. As the "position of a particle" is not an element of "physical reality", statements like "This vol-

ume of space is empty" cannot describe a physical reality either. Since (according to sec. 46) there is in any volume element of space a chance that a point event may happen, we can say, vaguely, that "matter is nowhere and everywhere". Certainly, we cannot say that atoms are small pieces of matter.

51. Operational Meaning of 'Matter'

A great part of this trouble comes from a misuse of the word 'matter'. In our everyday language we know very well what the operational meaning of 'matter' is. We know that iron or wood or the human body consist of "matter". But if we consider physical quantities which enter into the hypothetical setup of our physical science, we encounter words of which it is hard to say whether they mean "matter" or not. We use words like 'electricity', 'magnetism', 'ether', 'human mind', etc. The operational meaning of 'matter' is very clear if we speak of a piece of iron or wood or meat as being matter. Such a piece is identified as being "matter" by giving us the experience of resistance against penetration, of temperature, of color, of observable motion, etc. But if we ask whether we should call an electric charge or the ether "matter", we begin to doubt. For only a part of the operations which allow us to identify a piece of iron as matter are applicable in the case of an electric charge or of the ether. It is arbitrary to take just some particular operations as a characteristic of matter. The situation has been

dramatized by the fight which quite a few philosophers have put up in order to keep in use the word 'matter' without any regard for its operational meaning. It has been suggested that we have to distinguish between the physical concept of matter and the philosophical one. While in physics "structure of matter" means the way our observable bodies are built up from protons, electrons, etc., we are told that the philosophical concept of matter should mean everything which exists objectively independent of our subjective sense experience.

In order to avoid any ambiguity and to keep strictly to the operational meaning, it seems that the most reasonable thing to do is to use the word 'matter' in the sense it is used in our everyday language. This means to call a table a piece of matter and our brain a piece of matter, but not to refer by the word 'matter' to concepts like electrons or photons, let alone the "ether" or the "mind".

52. Electromagnetic Mass

At the end of the nineteenth century it became clear that every electrically charged particle has a mass, m, which can be calculated from the charge and the size of the particle. The mass m is (sec. 11) defined by $f = ma$. If we have a spherical particle with the charge e (in electrostatic units) and the radius a, the mass is approximately e^2/ac^2, where c is the speed of light. This follows from the well-known law of the electromagnetic field, according to which an accelerated particle is equivalent to an electric current of increasing intensity. According to the law of self-induction, every current has a certain inertia against increase of its intensity. Therefore, an electrically charged particle is "recalcitrant" against an attempt to accelerate it. Therefore, it possesses an "inert mass" according to the operational definition of this term.

Occasionally one has distinguished between a "real mass" in the Newtonian sense of this word and an "apparent mass" which is feigned by an electric charge. However, these names can be misleading. They may suggest the metaphysical idea that an "apparent mass" is nothing "material" but something more subtle, something immaterial—electricity. It has been suggested on good grounds that every mass may be an apparent one and have its origin in an electric charge. For a neutral particle could consist of a positive and a negative charge each of which would have an apparent mass. They would not reveal their charges, which are neutralized. This electromagnetic theory of matter has been described by the slogan "matter has disappeared". It has been used largely in a crusade against materialism and on behalf of idealism. It is noteworthy that Lenin made this crusade the starting-point of his principal philosophical book. Actually the word 'matter' does not mean anything beyond the realm of the operational meaning mentioned above. The "hard fact" is that every particle carrying a charge e has, according to the operational definition of

inert mass, a mass e^2/ac^2. Whether one has to introduce also a "mass" which is not covered by this formula is a question of the adjustment of our physical symbols to our experiences. It certainly has nothing to do with the question of the "reality" of matter.

53. The Number of Molecules in a Gas

To examine the structure of matter, it is obviously convenient to consider matter in the gaseous state. For, in this state, matter can be expanded arbitrarily and we can find out whether by this expansion we obtain the same type of matter in an infinitely diluted state or whether we obtain some small solid masses floating in a largely emptied space. If a gas is inclosed in a container, the phenomena taking place in it can be interpreted conveniently by the assumption that the gas consists of small particles which fly around until they hit the walls. The average kinetic energy is, according to the kinetic theory (sec. 19), proportional to the absolute temperature. These particles are called "molecules". They are certainly not pieces of "matter" in the sense in which we suggested in section 51 that this word be used. For such a particle has no temperature, no density, no color, but it has at every instant a momentum and a kinetic energy. It has certainly no surface as a piece of iron has, but it has a certain symmetry which may be spherical, cylindrical, etc. The salient point is whether the number of these molecules in one cubic centimeter can be figured out. What is the operational

meaning of this number? If there are several independent operations by which it can be figured out, we would say, with P. W. Bridgman, that these molecules are a "physical reality". This number can indeed be figured out. It can be done, e.g., by the observation of the kinetic energy of particles of microscopic visibility floating around in the gas and pushed around by the molecules. From the statistical hypothesis it can be derived that such a particle has the same kinetic energy as a gas molecule. Since we know the total energy of all molecules in the container from the observation of the pressure exerted upon the walls, we can divide this energy by the energy of a single molecule and obtain the number we are looking for. One obtains $L =$ approximately 6.8×10^{23} molecules in one mole (Avogadro's number).

Today there is even a method by which this number can be obtained in a much more direct way. In some cases one can spread out a substance in a layer which has the thickness of only one molecule. If we know the original volume of the substance and the area of the monomolecular layer, we can calculate the number of molecules. We obtain again the same number L. This number is the clue to the structure of matter. If we condense a gas, the molecules will touch each other approximately. We can obtain from the volume of the condensed state and the number L of the molecules the size of a molecule. We find that such a molecule is approximately

10^{-8} cm. across. This length—10^{-8} cm. —is a characteristic distance in the realm of molecules. It is called one "Ångstroem unit" (1 Å).

In a solid body, e.g., in a crystal, the molecules are almost in touch with each other. Therefore, the distance between two molecules in a solid body is also approximately equal to one Ångstroem. If the substance considered is not a chemical element but a compound, the molecule consists of a number of smaller particles called atoms. The table-salt molecule (sodium chloride, NaCl) consists of one atom of sodium and one atom of chlorine. The size of the atom is of the same order of magnitude as the size of a molecule. The molecule of a chemical element may consist of one atom, like mercury, or of several equal atoms, like hydrogen (H_2). The operational meaning of the number of molecules is very clear in a gas. In a container of the volume V at the temperature T, each molecule exerts the pressure KT/V on the wall, where K is a universal constant. In a solid body it is not unambiguous which atoms must be regarded as belonging to one and the same molecule. If we consider, e.g., a sodium chloride crystal, the sodium and chlorine atoms form a lattice within which there are no subdivisions. It is very arbitrary to pick just one sodium atom and one of the neighboring chlorine atoms and to call them a molecule. The statement that "one particular Na atom with one particular Cl atom forms a molecule" has no operational meaning.

54. The Problem of Chemical Binding

It has been an old problem to explain how the atoms in the chemical compound are kept in their position. A certain equilibrium between attraction and repulsion is needed. Is it possible to account for these actions on the basis of Newton's laws of motion and the laws of electromagnetic forces? The forces binding the atoms in the molecule are referred to as "valence forces". In the sodium chloride molecule the atoms are ionized. There is a positive and a negative electric charge. The attractive valence forces are electrostatic attractions between unlike charges. The repulsive forces are exerted by the shells of the atoms which are all negatively charged (secs. 58 and 59). But the matter is different if we have to derive the binding between the two hydrogen atoms in the hydrogen molecule. Between these neutral atoms there can be no attractive force in the Newtonian sense. The phenomenon of binding can be explained only on the basis of wave mechanics in which the concept of the path of a particle, and, in particular, of a particle at rest, has no operational meaning. This point will become clear when we discuss the structure of the atom (sec. 57).

55. The Structure of the Hydrogen Atom

We shall discuss first the simplest atom—the hydrogen atom—and describe later how the atoms of other elements differ from it. As already

mentioned, the hydrogen atom, which has a size of approximately 10^{-8} cm. = 1Å, consists of two smaller particles, the proton and the electron. It has been accepted as a fundamental hypothesis in physics that every electrical charge is an integer multiple of the elementary charge $e = 4.8 \times 10^{-10}$ e.s.u. The proton and electron are each charged with one elementary charge. But the electron has a negative, and the proton a positive, charge. If we know the charge, we can determine the mass of a particle by simple experiments on the basis of Newton's laws of motion. If we denote the acceleration by a, the mass by m, the intensity of the electrostatic field in which the particle is moving by E, Newton's law of motion says that $ma = eE$ or $a = e/mE$. By observing a and E, we can determine e/m, and, from knowing e, we can calculate m. Since we have good reason to identify the cathode rays with flying electrons, and the canal rays with flying protons, we find, by measuring the acceleration of these rays in electric and magnetic fields, that the mass of the electron is 9×10^{-28} gm., while the mass M of the proton is 1,837 times greater. These subatomic particles are, of course, much smaller size than the atom.

We can estimate their size in different ways. If we assume, e.g., that the mass m of the electron can be derived entirely from its charge, we have $e^2/r = mc^2$, where r is a radius of the electron (sec. 52). If we substitute into this formula for e the charge and for m

the mass of the electron, we find for the radius the value r = approx. 10^{-13} cm. We call the proton the "nucleus" of the hydrogen atom because its mass M is much greater (about 1,800 times greater) than the mass of the electron. According to the original presentation of atomic physics, due to Rutherford, the electron traverses an orbit around the nucleus. The nucleus is of a size similar to that of the electron. We shall discuss later how the nuclear size can be figured out. This presentation kept strictly to the language of Newton's mechanics. In this pattern of description the space between electron and nucleus is empty. There is no "matter" within the atom, and one could use this argument in favor of the philosophy of "dynamism", according to which there is no matter at all in the world but only centers of forces. The nucleus attracts the electron according to Coulomb's law, $e'e/r^2$, when e' is the charge of the nucleus. If we calculate the smallest orbit of the electron around the nucleus, we find again that this radius is approximately $r = 10^{-8}$ cm. (sec. 39). We found the same value for the size of the atom starting from the number L of molecules (sec. 53).

If we remember our presentation of wave mechanics, we must understand that it is a very perfunctory way of describing the hydrogen atom to say that the hydrogen atom consists of two particles which are very small compared with the diameter of the atoms, the space between them being empty.

As a matter of fact, these statements have operational meaning only within the range of Newton's mechanics; since the radius of curvature of the "orbit" of an electron would be comparable with the De Broglie wave-length (sec. 40), Newton's concept of the "path of a particle" has no operational meaning for an electronic orbit near the nucleus. The real description of the hydrogen atom is given by a function which describes to us the chance of finding the electron at a certain point in space if we make an experiment which could reveal the presence of an electron at this point. Or, speaking more exactly: If we bring into the vicinity of the nucleus a measuring instrument which is able to register point events (e.g., scintillations on a screen), we find that along the circle with the radius $r = 1$ Å such an event happens much more frequently than at other points. However, the event may happen at any point in space which is near to the nucleus. To say, therefore, that the space within the atom is "empty" is to use the word 'empty' in a sense which has no operational meaning. For the distinction between the part of space which is penetrated by a certain orbit and the part of space which is empty is a distinction which has an operational meaning only within the realm of Newtonian mechanics. Even the space "outside" the atom cannot be called "empty" in the original sense of this word. For at any point in space there is a slight chance that "the electron may be caught"; speaking exactly,

that a "point event" can happen (sec. 46).

56. Electron, Proton, and Neutron

The mass of the hydrogen atom is equal to the sum of the mass of the proton plus the mass of the electron. This sum is not very different from the mass of the proton itself. The chemical reactions of hydrogen are determined by the fact that its nucleus has a charge $+e$ and its shell consists of one electron with the charge $-e$. It was discovered recently that there is an element which has the same chemical properties as hydrogen but has a nucleus with a double mass. This means that the new element, "heavy hydrogen", has the same nuclear charge $+e$ and the same shell as ordinary hydrogen. But its nucleus contains besides the proton also a second particle which has nearly the same mass as the proton but does not carry any electric charge. This particle which has played a great role in recent nuclear physics is called "neutron". Ordinary hydrogen and "heavy hydrogen" are the simplest examples of a couple of "isotopes", i.e., atoms which have one and the same nuclear charge but different nuclear masses. Speaking accurately, the mass of the neutron is a little larger than the mass of the proton. Perhaps the neutron itself consists of one proton and one electron, for the charges of these particles would neutralize each other. If this were so, the neutron would be, in a certain way, a hydrogen atom on a smaller scale. The hydrogen atom consists of a proton and an electron at a

distance of 10^{-8} $cm.$ = 1 Å, but the neutron would consist of the same two particles but at a distance of only 10^{-12} cm. = 10^{-4} Å from each other (nuclear size). As we have seen, the smallest orbit of an electron around the proton has a distance of 10^{-8} cm. according to wave mechanics as derived from Coulomb's law of attraction and Bohr's theory of the atom.

The nucleus of heavy hydrogen is also called "deuteron". Its existence and stability give clear evidence that orbits of such a small size (10^{-12} cm.) cannot be derived from Newton's and Coulomb's laws. For the constituents of a deuteron, the positively charged proton and the electrically neutral neutron, do not exert any force upon each other according to the laws of mechanics and electrostatics. The fact that they can form a coherent particle, the deuteron, can be derived from the new mechanics of small particles. If we assume that the neutron consists of a proton and an electron, the deuteron consists of two protons and one electron. If we examine the field of force produced by both protons according to traditional physics, the electron could be at rest only in the symmetry plane of both positive charges. However, according to wave mechanics, there is for any space element a certain fraction of the time of observation during which the electron is staying in this region. Therefore, it can also be near to either of the positive charges. As a matter of fact, it can easily be shown that the electron changes periodically its position between the two

positive charges. 'Position of a particle' is to be interpreted in the complementary language of wave mechanics. Then each part of the system becomes alternately positive and negative. In this sense, the deuteron consists at any instant of time of two parts with different signs of charge. The sign of the charge on each side changes periodically. We call this way of attraction "exchange force", for this attraction is produced, vaguely speaking, from the fact that the electron changes its position among the two protons. These "exchange forces" are, of course, not "forces" in the strictly Newtonian sense. They determine no acceleration but only the frequency of a certain constellation of particles (speaking more exactly, of certain point events).

57. The Forces of Chemical Binding

The existence of the neutron and the deuteron throws some light upon the dark spots in the problem of valence forces in chemistry. As a nature is on the nuclear scale of 10^{-12} cm. a reproduction of the hydrogen atom, the deuteron nucleus is a reproduction of another well-known particle—the ionized hydrogen molecule. The hydrogen molecule consists of two hydrogen atoms, i.e., of two protons and two electrons. If we remove one electron, we obtain a particle with one elementary charge $+e$, called a positive "hydrogen molecule ion". It consists obviously of two protons and one electron exactly as does the deuteron nucleus. But, while this nucleus has a size of 10^{-12} cm., the ion is of atomic

size. The distance of the electron from the protons is approximately 10^{-8} cm. However, the existence of such a compound of two positive charges can be explained exactly as in the case of the neutron-proton compound by exchange forces. One can understand that the introduction of wave mechanics has contributed much toward the understanding of valence forces, the saturation of valences, and other problems of chemical binding (sec. 54).

58. The Structure of the Atoms of Different Chemical Elements

Two proton-neutron pairs form a new particle which has a charge $+2e$ and a mass of four proton masses. It is called a helium nucleus and is of nuclear size. If two electrons move around this nucleus at a distance of approximately 10^{-8} cm., we obtain a helium atom. The helium nucleus has been known for a long time as the alpha particle emitted by radioactive substances.

On the basis of this hypothesis about the structure of the simplest atoms, we can attack the ancient question about the structure of matter: Are the different chemical elements, iron, gold, sulphur, etc., really different substances, or are they only different configurations of one and the same substance? In physics this question has the following operational meaning: Is it possible to derive logically the chemical and physical properties of different elements like hydrogen, oxygen, iron, gold, etc., from the hypothesis that an atom of gold is

built up from exactly the same particles as an atom of oxygen, only in a different configuration? Previously the question was whether the atoms of gold, mercury, etc., are configurations of the simplest atom—the hydrogen atom. This seemed to be at least plausible. For the atomic mass of all elements is approximately an integer multiple of the atomic mass of hydrogen. Oxygen has an atomic mass which is approximately 16, nitrogen 14, and sulphur nearly 32 times the mass of the hydrogen atom. However, there are some serious exceptions. Chlorine, e.g., has the atomic mass 35.457. But exact measurements have shown that even the apparently integer numbers are only approximately integers. Nitrogen, e.g., is not exactly 14, but 14.008. Since we know that the atom itself is a compound of subatomic particles, at least of protons and neutrons, we understand that it was an oversimplification to expect all atoms to be just different configurations of hydrogen atoms.

We can expect only that all nuclei are compounds of neutrons and protons. Then, of course, the manifold of possible configurations is much greater, and we must not expect that the atomic masses are all integer multiples of the hydrogen mass. The generally accepted hypothesis is now that every atom has a nucleus which consists of N neutrons and P protons. They are packed so tightly together that they form a particle of nuclear size 10^{-12} cm. or 10^{-4} Å. The electric charge of such a nucleus is obviously

$+Pe$ and the mass approximately $P + N$ proton masses. This nucleus is surrounded at a distance of about 10^{-8} cm. by P negatively charged electrons. The total charge of the atom is zero. It is a neutral particle. Each nucleus is characterized by two integer numbers: the "atomic number" P and the "mass number" $A = P + N$. For the "ordinary hydrogen" $P = 1$, $A = 1$, for the "heavy hydrogen" $P = 1$, $A = 2$, for helium $P = 2$, $A = 4$, for "ordinary nitrogen" $P = 7$, $A = 14$, etc. As there is besides the "ordinary hydrogen" the isotope "heavy hydrogen" with the same P and a different A, there are besides the "ordinary nitrogen" several "isotopes of nitrogen" which have all the number $P = 7$ protons in common but have "atomic masses" A different from 14.

59. Chemical Properties and Atomic Weights

The chemical properties of an element, e.g., oxygen, depend upon the nuclear charge $+Pe$, which gives us also the number P of the electrons in the shell. Speaking exactly, the electrons are distributed among several shells. But the distance of the nearest shell from the nucleus is of the order of magnitude of 10^{-8} cm. Without going into details, we understand that chemical reactions take place in the following way: If two atoms (e.g., one sodium and one chlorine atom) approach each other, one electron of the outer shell of chlorine leaves its place and enters the outer shell of sodium. In this way these two atoms become of unlike electric charge (are ionized) and attract each other.

The chemical reactions of atoms are not affected by the number N of neutrons in the nucleus, provided that the number P of protons in the nucleus and electrons in the shell is not altered. We know already, as an example, the heavy hydrogen as distinct from the ordinary hydrogen. If atoms have equal nuclear charges Pe (i.e., equal atomic numbers P) but different numbers N of neutrons in the nucleus, they are "isotopes" and have different "mass numbers" A. Besides the ordinary oxygen $P = 8$, $A = 16$, there are isotopes with $P = 8$, but $A = 15$ and $A = 17$.

Two isotopes cannot be separated by chemical reactions. But as the masses of the nuclei are different, they can be separated by physical methods which make use of this difference, e.g., diffusion and centrifuging. If we have a piece of iron or a container filled with chlorine, we have obtained these samples by purifying them by chemical methods. But it can happen that our sample which is chemically pure contains not only atoms with one and the same number of neutrons but perhaps a mixture of isotopes which contain all the same number of electrons P which is characteristic of this chemical element. But the number of neutrons is different, and therefore the mass numbers are different too. If we have, e.g., a mixture of x atoms of ordinary hydrogen and y atoms of heavy hydrogen, the mass of the mixture would be $x + 2y = x(1 + 2y/x)$

proton masses. If y/x is, e.g., 1/10, the mass of the mixture would be $1.2x$ proton masses. The number $1.2x$ is called the "atomic mass" of the mixture. It is the same number which is known from elementary chemistry as the "atomic weight" of an element like hydrogen. Actually it is the mass of a mixture of several isotopes of hydrogen. This "atomic mass" is certainly not an integer multiple of x. The departure of the atomic mass of elements from the integer multiples of hydrogen can be understood if we assume that, e.g., the gas which we call chlorine is actually a mixture of several isotopes of the ordinary chlorine. The departure from the integer value depends upon the composition y/x of this mixture.

However, even if we consider a chemical substance which is not a mixture of isotopes, the atomic mass cannot be expected to be an exact integer multiple of the proton mass (sec. 30). The "atomic mass" is not exactly equal to the "mass number". For example, in the case of helium ($A = 4$) we know that the nucleus consists of exactly four particles. If they were separated, their "atomic mass" would be "four". But by the formation of the helium nucleus potential energy decreases, kinetic energy is produced, and the rest mass must decrease (sec. 30). We know from the theory of relativity that the tight packing of the protons and neutrons in the nucleus must be responsible for a mass defect. The mass of the nucleus must be smaller than the sum of the masses of the protons and neutrons which constitute the nucleus. This fact accounts for the small departure of the atomic masses of chemical elements from the exact multiples of the proton mass, while the mixture of isotopes accounts for the "great" departures.

60. The Nuclear Forces

If we bombard atoms (e.g., a foil of gold) with alpha particles (helium nuclei with a charge $2e$), these particles are deflected by the repulsion of the positively charged gold nucleus. From the examination of the hyperbolic orbits of the particles under the influence of the deflecting forces we notice that this deflection is in some cases so strong that we have to assume that the alpha particles get very close to the center of the gold nucleus. Since the repulsive force is the Coulomb force (Pe^2/r^2), we can find from an easy calculation that the particle must approach the center of the gold nucleus to a distance which is smaller than 10^{-12} cm. One can also find from this calculation that for a distance greater than 10^{-12} cm. the Coulomb inverse-square law accounts well for the observed orbit. But it is equally certain that at a distance of 10^{-12} cm. from the center the inverse-square law is no longer valid. At a distance of 10^{-13} cm. a new type of force comes into play—the "nuclear force".

We have to do here not with a "force" in the Newtonian sense but rather with a kind of "exchange force". These "forces" are negligible

at a distance which is considerably more than 10^{-13} cm. from the center. The radius of the electron can be calculated (sec. 52) as $a = e^2/mc^2 = 10^{-13}$ cm., if we assume that the mass of the electron has its origin only in its electric charge. If we assumed the same thing for the proton, its radius would be e^2/Mc^2, where M is the mass of the proton: $M = 1{,}837$ m. Therefore, the size of the proton would be only about 10^{-16} cm. The size of the nuclei would be much smaller than the size of the electrons. However, the examination of the deviation of alpha particles by the nuclei of different substances shows that the size of the nucleus cannot be so small. It must be, as a matter of fact, of the order of magnitude of 10^{-13} cm. This means it must be approximately the same as the size of the electron. This consideration shows that the proton mass cannot be completely derived from the electric charge; there must be forces other than the electromagnetic ones. These nuclear forces, which are analogous to the chemical valence forces, distinguish the phenomena in the nucleus from the phenomena at a great distance.

The chemical reactions and the emission of spectral lines (of visible light and X-rays) are phenomena in the shell or, exactly speaking, phenomena which can be predicted from what happens in the shell. However, there are other phenomena which have their foundations in the happenings within the nucleus.

61. The Phenomena of Radioactivity

The spontaneous emission of alpha particles by some atoms, in particular the radium atom, was one of the earliest discoveries which foreshadowed the twentieth-century physics. This emission is the first example of an atomic phenomenon which originates in the nucleus and cannot be understood on the basis of Newton's mechanics. According to section 46, it can be derived from wave mechanics. From this theory we can predict the percentage of alpha particles which leave the nucleus per second. But we cannot predict which specific particle will succeed in leaving the nucleus. It has been known for a long time that no physical law predicts which alpha particle will leave the nucleus but only what percentage of all particles present in the nucleus are emitted per second. This percentage accounts for the rate of decay which is characteristic for every radioactive substance.

The radioactive phenomena which were originally regarded as peculiar exceptions to the well-known laws of physics have now become a typical example of the application of wave mechanics. Originally one knew only an emission of alpha particles to be a spontaneous reaction within the nucleus of radioactive atoms which occurs without any interference from without. But later it was discovered that disintegration of nuclei can be produced artificially if we bombard nuclei with protons, neutrons, or alpha particles. The "stable" nuclei can be

disintegrated by bombardment, while the unstable (radioactive) ones disintegrate spontaneously. It was also discovered that by bombardment a stable nucleus can be converted into an unstable one (artificial radioactivity).

62. Production of Positrons, Electrons, and Photons by Nuclear Reactions

As a result of bombardment experiments not only the particles which have been known before can emerge but a new particle was discovered, the "positron," which has the positive electronic charge $+e$ and a mass which is as small as the electron mass.

Whereas protons and neutrons are believed to be present in the nuclei of all chemical elements, the electrons, positrons, and photons which are emitted by the nucleus during a great many reactions have not been parts of the nucleus before but are produced by the reaction. We know that the atom emits light (photons) when its energy drops, and nonetheless these photons have not been present in the atom before. In the same way the nucleus emits photons which are produced by the transition from one state of the nucleus into another state with a lower energy. The photons emitted by the nucleus are, however, of a much higher frequency than the light emitted by the atoms. The "nuclear photons" are the "gamma rays" which are known from radioactive radiation.

These considerations make it very clear that the nuclear reactions can-

not be described by using the language of the mechanics of everyday life. We cannot simply say that the beta particles or the alpha particles are emitted from the nucleus as bullets are shot from a gun. Even the removal of a particle by bombardment cannot be described in this language of the mechanics of medium-sized bodies. According to Niels Bohr, a particle (e.g., a neutron) which strikes the nucleus sticks first in the nucleus like a bullet fired in a box of sand. Then a reaction takes place between the intruding particles and the nucleus. By this reaction new particles are produced. The intruding particle does not simply throw out other particles as a ruffian intruding in a crowded streetcar would throw out other passengers. The emission of a beta particle (electron) from the nucleus follows laws which are more involved. If the nucleus experiences a certain energy drop ΔE, a photon (gamma ray) of a definite frequency ν is emitted. ν is determined by $h\nu = \Delta E$ (sec. 39). The beta particles which are emitted have a continuum of kinetic energies.

63. The Structure of the Nucleus

If we try to make a picture of the structure of the nucleus, we can do it at least in a perfunctory way by using the model of a "drop of fluid" suggested by Bohr. The forces by which the nucleus is kept together do not belong to the electromagnetic type but to the exchange forces which account for a great part of chemical binding between the atoms in a mole-

cule and for the cohesion between the atoms and molecules in a piece of matter.

These nuclear forces produce an effect which can be compared to the surface tension which is responsible for the cohesion of a drop of fluid. On the other hand, the positively charged protons exert repulsive forces upon each other. A stable nucleus can exist only if there is equilibrium between this "surface tension" which is proportional to the area of the surface and the repulsive forces of the protons.

If we consider a nucleus with few protons and neutrons, we notice that the number of protons and neutrons are almost equal. For light nuclei we find approximately $P = N$ or $A = 2P$. For oxygen, e.g., we have that $P = 8$. The atomic mass A is for "ordinary oxygen" exactly $A = 16$. There are, however, isotopes with $A = 17$ and $A = 18$, where $A = 2P$ is only approximately fulfilled. We can conclude, therefore, that for these light nuclei surface tension and electrostatic repulsion are in equilibrium if the number of neutrons equals approximately the number of protons. But, if we consider heavy nuclei, the area of their surface by unit of volume becomes larger. Therefore, the number of neutrons which are needed to produce a sufficient surface tension becomes greater. In the case of the heaviest nucleus ($P = 92$) there are 146 neutrons in the "ordinary uranium," while for a rare isotope we find $N = 143$. This means that the mass numbers are $A = 238$ and $A = 235$,

which are both considerably greater than $2P = 184$.

It is obvious, therefore, that by splitting a heavy nucleus, such as uranium, into two lighter nuclei, some of the neutrons are liberated, as they are not needed to secure the stability of the lighter nucleus with $P = 90$ or greater. There is no stable nucleus with $P = 90$ and greater.

As we know from the theory of relativity, the "binding energy" which keeps the particles together is connected with the "mass defect" of the nucleus (sec. 30). If we compare the sum of the masses of the particles (protons and neutrons) with the measured atomic weight of a chemical element, we find a certain difference which is due to the energy which has been used in the formation of the nucleus. If we assume that we have to do not with a mixture of isotopes but with a sample which consists of identical nuclei of the atomic number P and the mass number A, the atomic weight W which is measured would be $W = A = P + N$, if we computed the atomic weight by adding up the masses of the particles from which the nucleus is built (neglecting the small difference between proton mass M and neutron mass). As a matter of fact, the measured atomic weight W is a little smaller than the mass number A. If this mass defect is $(A - W)M$, the "binding energy" of the nucleus B is $B = c^2 M(A - W)$, which has to be supplied in order to break up the nucleus into its elementary particles. This energy is also liberated by the formation of the nucleus.

It is particularly important to know the binding energy of a nucleus divided by the number of particles of which this nucleus is built. This number B/A has, in first approximation, one and the same value for all nuclei. This fact checks well with the picture of the nucleus as a drop of fluid. For in a fluid the energy is proportional to the volume, and the volume of a nucleus is proportional to the number A of particles.

If we examine the values of B/A more closely, we notice a clear trend. The "binding energy per particle" is increasing from the lightest nuclei to the value $A = 60$ (nickel) and is again decreasing toward the heavy nuclei. The nuclei in the middle of the series are the most strongly bound.

If we speak of a nucleus being comparable to a drop of fluid, we do not mean, of course, that the behavior of a nucleus can be predicted exactly by the equations of fluid motions as treated in ordinary hydrodynamics. For this would mean that the particles in the nucleus obey Newton's laws, which is outruled by the experiments on the behavior of small bodies (Part VII). The similarity to a fluid is restricted to the validity of some algebraical and dimensional relations which have the same form as the relations between surface tension and repulsive forces in a fluid. It is noteworthy, however, that the behavior of these extremely small particles can be predicted, as far as some general features are concerned, by this picture. It would, however, be a great mistake

if anyone should take this picture literally and draw conclusions from Newton's laws in an attempt to show that the motions of fluid particles contradict the behavior of small particles as predicted by wave mechanics. One must never forget that in the term 'particle' in the description of the nucleus has not the same operational meaning as this term has in ordinary mechanics. 'Particle' in nuclear physics is only an abbreviation which refers to "point events" which can be statistically predicted.

64. Atomic Power[20]

We learned from section 63 that the formation or the breaking-up of a nucleus can become a source of energy (atomic power). We consider not only the formation of a nucleus from the "elementary particles", protons and neutrons, but from any smaller nuclei. If the binding energy of the resulting nucleus is smaller than the sum of the binding energies of the "building stones", the formation produces energy and rest mass disappears. If, however, the binding energy of the resulting nucleus is greater than the energy sum of the constituents, energy is liberated in breaking up this nucleus, and the fragments together have a smaller mass than the original nucleus.

If A increases, the binding energy per particle, B/A, decreases among the lighter nuclei and increases among the heavier nuclei. By this fact two ways are suggested for the production of energy from nuclear reactions. One

could either use the formation of a light nucleus or the disintegration of a heavy nucleus.

The first way was used by nature in our sun, the second one by human inventors in the "atomic bomb". According to a widely accepted astrophysical theory, the heat of our sun is reproduced by a cycle of nuclear reactions (Bethe's Carbon Cycle) which boils down eventually to the formation of helium nuclei from protons.

In the first atomic bomb an isotope of uranium ($P = 92$, $A = 235$) was used. This heavy nucleus can be broken up by neutron bombardment. Every uranium nucleus of 235 particles is split into two nuclei of nearly equal size and some free neutrons. As the binding energy of all the fragments together is much smaller than the energy of the original uranium nucleus, there is a considerable production of kinetic and radiant energy accompanied by a disappearance of rest mass. The liberated neutrons hit other uranium ($A = 235$) nuclei and produce a further disintegration. In this way a "chain reaction" is started, which produces in a short time a considerable amount of energy in a way similar to that in which a lighted match can start a forest fire, by a "self-sustained" reaction.

This kind of splitting into two particles of similar size has been known under the name of "uranium fission". In order to start this chain reaction, one does not need to produce neutron projectiles first. There are always some neutrons in the air, originating

from cosmic rays or radioactive substances. According to wave mechanics (sec. 46), there is even a certain chance that fission starts spontaneously. By the disintegration of 1 pound of uranium (235) an energy of nearly eleven million kilowatt hours is produced.

The process which takes place in the atomic bomb is a good example to offer in showing that disintegration of nuclei is a very common phenomenon which is not restricted to the radioactive processes in the older sense (emission of alpha and beta particles).

Besides the uranium bomb, a second type has been used which provides a good example of fission combined with artificial radioactivity. If ordinary uranium ($P = 92$, $A = 238$) is bombarded with neutrons, sometimes a neutron is captured by a nucleus. When this happens, a radioactive emission (of beta particles) takes place. This can be interpreted by assuming that one neutron in the nucleus emits an electron and is converted into a proton (sec. 62). The number of protons P now becomes 93, and we have a new element "neptunium". By again capturing a neutron, and again emitting an electron, the neptunium becomes "plutonium", with $P = 94$. This new element is the material of the second atomic bomb. It suffers fission like uranium ($P = 235$) and can start a chain reaction.

The explosion of the bomb has proved that Einstein's law $E = m_0 c^2$ (sec. 30) plays a role not only in subtle laboratory experiments but also in power engineering. The introduction

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of the speed of light c into the equation of mechanics, which has been thought of as a far-fetched sophistication, becomes now conspicuous by the enormous energy production which can be understood only by the enormous value of c^2, the square of light velocity.

The explosion of the atomic bomb provided a very conspicuous example of the possibility that "mass can disappear". As a matter of fact, the disintegration of one pound of uranium ($P = 235$) left only 0.999 of a pound of fragments, while 0.001 of a pound was "converted into energy" (radiant and kinetic). We must not forget, however, that it would be misleading to describe this fact by saying that "matter has disappeared". We can say this if we mean "rest mass" by the word 'matter'. But if we gave to the sum of rest mass and E/c^2 the name 'matter', we could say again that "matter was conserved" even through the atomic explosion. If we use the term 'matter' beyond its common-sense meaning (sec. 51), we must give it an arbitrary operational definition. As in the case of 'time' or 'length', some operations which are to measure the "quantity of matter" yield identical results, if we use the word 'matter' in the traditional sense and assume that the laws of traditional physics are valid. But if the laws of physics were different, it might happen that these operations would render different results, and it becomes ambiguous which of them is the "real quantity of matter". An example of such operations is provided by the measuring of masses before and after an atomic explosion. If the relativistic mechanics is true, the "masses" before and after the explosions are not equal. Therefore, "matter" has disappeared, if we identify "matter" with "rest mass". But if we choose a different operational definition of matter, we could not say that "matter has disappeared".

This statement has only an unambiguous operational meaning if we stick to traditional physics and its terminology. But in the new physics we cannot say whether "matter" does disappear or not, because the word 'matter' can no longer be used unambiguously, without introducing a new operational definition. As we learned in section 2, the operational meaning of an expression can only be defined unambiguously if we assume the validity of certain physical laws.

IX. Conclusion

It is hardly possible to decide accurately whether a certain presentation of physics is fit to become a part of a unified presentation of the sciences. However, it is easy to ascertain that quite a few presentations of physics have decidedly produced confusion when one has tried to fit them into the structure of other fields of knowledge.

In biology, for instance, the physics of the twentieth century has been used to bring about a decision in the age-old conflict between the "mechanistic" and the "vitalistic or organismic" conception. We have been told that the "new physics" decides in favor of spontaneity and emergent evolution and against rigid mechanical explanations.

In medicine the "new physics" has been quoted as favoring the case of all sorts of "practitioners" in their old fight against "orthodox school medicine". For, by the principle of uncertainty, a certain flexible element has been brought into science which, according to these people, eases the rigidity of scientific rules and provides a place for methods which are based more on intuition than on systematic knowledge.

In sociology and economics the new physics has been employed to bolster the "organismic view" of human society sponsored by some religious and political groups against the "mechanistic view of society" which has been allegedly the view of liberal and Marxist economists.

In the philosophy the new physics has been put into the service of the fight of religion against materialism by pointing out that now "freedom of will" has won a certain place in the formerly rigidly deterministic universe.

Even the "occult sciences" have seen a green light in the physics of the twentieth century. The theory of relativity introduced the conversion of matter into energy and has supported the belief in "dematerialization".

As we learned in Part II, every physical theory consists of three kinds of statement: equations between physical quantities (relations between symbols), logical rules, and semantical rules (operational definitions).

The sensational results of the application of the "new physics" to biology, sociology, medicine, etc., have been very frequently achieved by the following method: The symbols of physics (e.g., waves and particles) have been inserted into the sciences of biology, sociology, medicine, etc., but the operational definitions of the symbols have been omitted. This means that the physical theories have been applied in a crippled condition. As symbols without operational meaning do not lead to any palpable result, some kind of operational definition had to be introduced. Actually, those symbols have been interpreted as meaning what they traditionally mean in biology or sociology. This meaning is, of course, very different from their operational meaning in physics.

We see that the application of the "new physics" to other fields of knowledge has not always been made by fitting the physical theories in their scientific form to the structure of other sciences. No wonder then that nothing but confusion could be the outcome of such an attempt to "integrate human knowledge".

Examples are obvious. If we say in physics that in the realm of the subatomic phenomena the concepts of

mechanics cannot be applied, we mean to say that the symbols of classical mechanics, like 'position of a particle', have no operational meaning in the realm of the subatomic phenomena. However, in biology there has been an ancient conflict between the mechanistic and the vitalistic conception. To say that the "conceptions of mechanics cannot be applied to the phenomena of life" has always meant that life is something autonomous, spontaneous, emergent, etc. The symbols of the new subatomic physics, if understood in connection with their original operational meaning, allow a description of the subatomic phenomena which has nothing to do with spontaneity, emergent evolution, or purposiveness. This "new physics" is, in the language of biology, not less "mechanistic" than classical physics. Therefore, if we understand all statements of physics in their operational meaning, we cannot draw any conclusion in favor of a vitalistic biology.

If the statement "length is relative" is understood in its operational meaning, it is a statement about the fact that certain procedures of measurement yield different results, whereas it had formerly been believed that they yield identical results. But if we transplant the statement "length is relative" without its operational meaning into psychology or sociology or medicine, the word 'relative' is interpreted, of course, in the way in which this word has been traditionally used in these fields of knowledge. It has meant that all knowledge is subjective or historically and ethnically conditioned. If we carried the original operational meaning of "relativity" from physics into psychology or medicine, we could never arrive at a statement about subjectivity or vagueness, let alone "agnosticism".

What we can learn from these examples is simply the fact that a presentation of physics is fit to be a part of the unified sciences only if the operational meaning of every statement is explicitly formulated and carefully carried along when the statement is applied to other sciences.

While a physicist works within his own domain, the operational meaning of his statements is "understood". No explicit logical analysis is needed, and such an analysis would even be tiresome and pedantic. When, however, a statement of physics is transplanted into a foreign soil, the operational meaning is no longer "understood", and a careful logical analysis is indispensable. Otherwise, we have to disprove statements like: "The first explosion of an atomic bomb exploded once forever the doctrine of the materialists" or "was a crucial experiment in favor of dematerialisation" or "spiritualism is confirmed by science".

While one lives within a limited group of people, it is "understood" what we mean by calling a man a "nice fellow"; but if one comes in contact with an unknown group, there is not even a guess as to whether by calling a man a "nice fellow" we hint that he goes all-out for prohibition or that he likes a good drink.

In a presentation of the "new physics" we must, therefore, distinguish very carefully between statements which ascribe to words or symbols a new operational meaning and statements which describe the results of new experiments.

But we must not fail, on the other hand, to understand the close interconnection between these two kinds of statement. As we learned in Part II, and confirmed by the examples of Parts V (relativity) and VII (wave mechanics), symbols and expressions are introduced only if they are of some help in formulating physical laws. This means that physical operations can serve as definitions of physical quantities only if a great many operations lead to an identical numerical result. "In the discovery of what operations may be usefully employed in describing nature is buried almost all physical experience. . . . . The discovery that the number obtained by counting the number of times a stick may be applied to an object can be simply used in describing natural phenomena was one of the most important discoveries ever made by man".[29]

As we learned in the Introduction, quite a few physicists have suggested that the misuse of physics in other fields of knowledge could be prevented by isolating physics from any contact with people who want to interpret its meaning for human thought in general. By such an ostrich policy physics would be converted into a collection

of rules which might be useful in technology. But physical science would lose its traditional role as the vanguard of progressive thought.

A danger of this kind has arisen again and again in the history of thought. In the year 1907 the French philosopher and historian of science Abel Rey wrote: "If physical science which has had an essentially emancipating effect in history goes down in a crisis which leaves it only with the significance of a technically useful collection but robs it of every value in connection with the cognition of nature, this must bring about a complete revolution in the logical art. The emancipation of the mind, as we owe it to physics, is a most erroneous idea. . . . . One must restore to a mystical sense of reality everything that we believed had been taken away from it"[30]

This danger cannot be met by isolationism in physics. The only thing to do is to make "physics safe for its role as a part of the unified sciences". This means again to give careful thought to the logical analysis of physics and to be always aware that only the combination of relations between symbols, logical rules, and operational definitions constitute the science of physics. There are quite a few philosophers who have felt sorry that "behaviorism" and "logical empiricism" have banned words like 'soul' and 'mind' from scientific psychology. They may find comfort in the fact that the same schools of thought have banned words like 'matter' from scientific physics. A "soulless" psychology and

a "matterless" physics have been established as parts of "Unified Science". Words like 'matter' and 'mind' are left to the language of every-

day life where they have their legitimate place and are understood by the famous "man in the street" unambiguously.[21]

Notes

1. "I have not attempted to introduce a new philosophy into science, but rather to remove an obsolete philosophy which has occasionally survived in the writings of scientists for a longer time than in the writings of the philosophers themselves" (Ernst Mach, *Erkenntnis und Irrtum* [1905]). "To neglect philosophy when engaged in the re-formation of ideas is to assume the absolute correctness of the chance philosophic prejudices imbibed from a nurse or a schoolmaster or current modes of expression" (A. N. Whitehead, *The Principle of Relativity* [1922]).

2. Philipp Frank, "Why Do Scientists and Philosophers So Often Disagree?" *Review of Modern Physics*, Vol. XIII (1941), and "The Philosophical Meaning of the Copernican Revolution," *Proceedings of the American Philosophical Society*, Vol. CLXXXVIII (1944).

3. W. A. Wisch, *Metaphysics and the New Logic* (1942); A. C. Benjamin, "The Unholy Alliance between Operationalism and Positivism," *Journal of Philosophy*, Vol. XXXIX (1942).

4. This *Encyclopedia*, Vol. I, No. 3, § 23 (Carnap).

5. *Ibid.*, § 8.

6. *Ibid.*, § 4.

7. *Ibid.*, § 24.

8. P. W. Bridgman, *Logic of Modern Physics* (1927).

9. It has been suggested by some authors that some rules of traditional logic be altered in order to fit modern quantum mechanics into a formalized system: M. Strauss, in *Erkenntnis*, Vol. VI (1936); L. Rougier, "La Relativité de la logique," *Journal of Unified Science*, Vol. VIII (1936); and G. Birkhoff and J. v. Neumann, "The Logic of Quantum Mechanics," *Annals of Mathematics*, Vol. XXXVII (1936).

10. Bertrand Russell, *The Scientific Outlook* (1931).

11. P. W. Bridgman, *The Nature of Thermodynamics* (1941).

12. Conventionalism has been the starting-point of a great many attempts to justify metaphysical creeds by arguing that "science" cannot tell us anything about physical reality and that therefore a vacuum is produced within which extrascientific methods may operate at will (see E. Le Roy, *Rev. de met. et de mor.*, Vols. XVII and XVIII [1899, 1900]; Pierre Duhem, "Physique de Croyant," *Annales de phil. chrét.* [1905/6]). The scientific angle is stressed by E. Nagel, "Nature and Convention," *Journal of Philosophy*, Vol. XXVI (1929).

13. Henri Poincaré, *Science et hypothèse* (1903); P. Duhem, *La Theorie physique, son objet et sa structure* (1926); Philipp Frank, *Le Principe de causalité et ses limites* (1937), p. 184; and P. W. Bridgman, *The Nature of Thermodynamics.*

14. P. Frank, *Le Principe de causalité et ses limites*, p. 73.

15. P. Frank, *La Fin de la physique mécaniste* (1936).

16. P. Frank, "The Mechanical vs. the Mathematical Conception of Nature," *Philosophy of Science*, Vol. IV (1937).

17. P. Frank, "Modern Physics and Common Sense," *Scripta mathematica* (1939).

18. P. Frank, "Relativity and Its Astronomical Applications," *Sky and Telescope*, 1942, p. 9.

19. R. v. Mises, "Die Krise der Mechanik," *Proc. Congr. f. Appl. Mech.* (Delft), 1924.

20. *Logic of Modern Physics*, p. 59.

21. "It is the recoverability of the original situation that is important, not the detailed reversal of the steps which led to the original departure from the initial situation" (Bridgman, *Nature of Thermodynamics*, p. 122). In a great many writings this distinction has not been made and the word "reversibility" is used for "recoverability."

22. Lord Kelvin (1852) cautiously assumed the validity of the Second Law for "inanimate" systems only. For a discussion of the apparent conflict between organic evolution and increase of entropy see Lecomte du Nouy, *Biological Time* (1936), and Bridgman, *Nature of Thermodynamics*, pp. 208 ff.

23. Bridgman, *Nature of Thermodynamics*, pp. 148 ff.

24. Quite a few philosophers suggested the introduction of a "philosophical time concept", without a clear-cut operational meaning, in addition to the time concept based on physical operations as used in the theory of relativity: H. Bergson, *Durée et simultanéité* (1922); A. O. Lovejoy, "The Time-retarding Journey," *Philosophical Review* (1931). A time concept based on biological operations was introduced by Lecomte du Nouy, *op. cit.*

25. "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" *Physical Review*, Vol. XLVIII (1935).

26. In Newtonian mechanics the operational meaning of 'force' is based on the measurement of the acceleration of a particle. In the mechanics of

subatomic particles the operational meaning of 'force' can be reduced to the statistical distribution of point-events or impulse-events in space at a certain time.

27. Albert Einstein, "Physics and Reality," *Franklin Institute Journal*, Vol. CCXXI (1936).

28. H.D.Smyth, *Atomic Energy for Military Purposes* (Princeton, 1945).

29. P. W. Bridgman, *The Logic of Modern Physics*, p. 27.

30. *La théorie de la physique chez les physiciens contemporains* (Paris, 1907).

31. "Philosophy deals with positive truth, yet contents itself with observations such as come within the range of every man's normal experience" (C. S. Pierce, *Collected Works*, Vol. I, sec. 241).

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Cosmology

E. Finlay-Freundlich

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Contents:

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E. Finlay-Freundlich

1. Introduction

1,1. In cosmology an attempt is made to answer several questions. How is the universe built up as a whole? Do the laws of nature, which we derive from experience gained in our "neighborhood"—be this the earth or the solar system or the galaxy—remain applicable if we imagine this "neighborhood" extended until it comprises the whole universe? Is an infinite world compatible with the laws of nature or must we restrict the world to a finite size if we wish to avoid insurmountable difficulties of principle?

Cosmology has to face the problem of infinity not as a mathematical abstraction but as a physical reality. This confers on all its problems a particular speculative character and, at the same time, a particular attraction; for many of us are anxious to obtain an answer to the question: Can we form a picture of the whole universe? Naturally, to such a question a definite and final answer cannot be expected in the near future, and it is not surprising that all attempts so far have come to an early standstill on account of our much too limited and scanty knowledge. Despite that, attempts to frame a unified picture of the whole world have not ceased, and they have been of great scientific value because they have considerably widened and deepened our understanding of the fundamental laws of nature.

1,2. Remarks on the History of the Problem

From 1917 on, cosmology has again been in the foreground of interest in connection with the development of the general theory of relativity. It seemed as if this theory could bring us a decisive step nearer to a solution of the cosmological problem. The actual situation, however, is not so simple. It is true that

cosmology has taken a decisive step forward during the past few years, but this advance was predominantly due to the amazing advance of astronomical knowledge beyond the realm of our galaxy into remote depths of the universe. Our "neighborhood," until then confined to small parts of the galaxy, was extended, by a rapid advance into extragalactic space, to distances which possibly may no longer be considered negligibly small as compared with the dimensions of the whole universe. The cosmological problem took full advantage of this enrichment of our knowledge. The theory of general relativity at the same time opened an incomparably deeper insight into its foundations. As far as the representation of observational facts is concerned, classical mechanics, no less than the theory of relativity, offers explanations for practically all observations. In this respect neither of them may claim to be definitely superior to the other. But the theory of relativity discloses the full variety of possible solutions and promises in the end a very much deeper understanding of the structure of the universe.

The first idea, which later proved to be really essential for a solution of the cosmological problem, may be called Lambert's idea of a hierarchic structure of the universe (see 2,2). This idea has lost, in the present situation, its finality. But at an earlier stage it removed, when applied in the right way, the main obstacles which seemed to bar the first efforts to solve the cosmological problem within the framework of Newton's mechanics. These first efforts were based, as we now know, on empirical foundations much too narrow and weak for such a complex problem. Thus the possible solutions that were offered had a rather academic character and never fully satisfied the imaginations of natural scientists. As a result, cosmological considerations remained in the background until the great breakthrough into the world of spiral systems with its new discoveries gave a tremendous impetus to the whole problem. When that occurred, the guidance was taken over by the theory of relativity, which, no less than Newton's theory of gravitation, had to stand the test of its cosmological consequences and which offered more efficient tools to attack the whole problem.

2. Earlier Discussions of the Cosmological Problem on the Basis of Newton's Law of Gravitation

2,1. The Observational Background

Every investigation of the structure of the universe has to start from a knowledge of, or from assumptions about, the distribution of matter in space. And since obviously all experience will always be restricted to a part, probably to a small part, of the universe, no exact knowledge of the distribution of matter over the whole universe can ever be claimed. We therefore attempt to infer from the observed distribution in our "neighborhood" how matter may be distributed beyond the surveyed part of space. This is the basic assumption in all cosmological considerations.

As long as astronomy had not penetrated into extragalactic space beyond the boundaries of the star-system to which the sun belongs, stars were believed to be the elements, the "bricks," so to speak, of which the universe was built. We believed ourselves to be imbedded in an ocean of stars. From star distances derived by the astronomers in various ways, the law according to which the stars are distributed in space was inferred.

The most simple initial assumption was that of uniform distribution. If the stars were uniformly distributed in space and, in addition, were all of equal luminosity, i.e., of equal absolute magnitude,[1] or, alternatively, if the relative frequency of stars of different luminosities for every element of space were the same, their numbers—counted in order of increasing magnitudes m—should increase according to the formula:

$$\frac{N_{m+1}}{N_m} = 3.982 ,$$

or

$$\log N_m = 0.6 \cdot m + \text{constant} .$$

(2,1.1)

In this formula N_m measures the number of stars as bright as, or brighter than, the magnitude class m. The formula assumes, moreover, that space is transparent, so that the brightness of any light source decreases inversely with the square of its dis-

tance. The derivation of the formula can be briefly outlined as follows:

Suppose the star-systems to be uniformly distributed in space and all of equal luminosity; the number N_m of systems down to the limiting magnitude m is obviously proportional to the volume of the sphere on the surface of which a system reaches the limiting brightness m. Hence $N_m \sim r_m^3$, where r_m denotes the limiting distance. On the other hand, since the inverse-square law for the decrease of apparent brightness i_m with distance is assumed to hold, we have

$$\frac{i_m}{i_{m+1}} = \frac{r_{m+1}^2}{r_m^2} = 2.5 ;$$

for, by definition, the ratio of intensity of two light sources differing by 1 magnitude in apparent brightness is equal to 2.5. Consequently,

$$\frac{N_{m+1}}{N_m} = \frac{r_{m+1}^3}{r_m^3} = (2.5)^{3/2} = 3.982$$

(or roughly $= 4$) and this, since

$$N_m = 3.982^m N_0 ,$$

yields (N_0 being a constant)

$$\log N_m = 0.6 + \text{const} .$$

It was soon realized that in the star-system surrounding us, i.e., the galaxy, the assumption of a uniform distribution does not hold good; and the correct conclusion was drawn that the galactic system is of finite size. Despite that, the preceding formula still dominates all cosmological considerations. However, it is no longer applied to the distribution of stars in the galaxy but to that of star-systems in the extragalactic space, now that it has been realized that star-systems are the elements from which the universe is built up. All statistical data concerning the distribution of matter in space in the following cosmological considerations refer to the distribution of star-systems in the universe and will be based on formula (2,1.1).

For the moment only the most simple case of uniform dis-

tribution of matter has been considered. If space is infinite and the density of matter everywhere finite, i.e., not zero, an infinite world, filled with an infinite amount of gravitating matter, would result. Is such a universe conceivable? Are we permitted to perform this transition to infinity without encountering difficulties of principle?

2,2. Olbers' and Seeliger's Objection to Such a Universe

In a world in which the mean density ρ of matter is finite, even if its value is as small as we please, the limit of the gravitational potential

$$\phi = \int_V \rho \, \frac{dV}{r} \qquad (2,2.1)$$

has no definite value when V, the volume, tends to infinity.[2] Also the expression for the gravitational stress becomes indefinite. Seeliger's objection against a universe filled with an infinite amount of matter is based on this fact.

If the world contained infinitely many bright stars, the night sky would shine with a brightness corresponding to the average surface brightness of these stars; this is Olbers' objection.

Both objections can be removed by applying the idea of a hierarchic structure of the world which prescribes a special distribution of the infinite amount of matter over the infinite Euclidean space. The singularities mentioned above may thus be avoided. By "a hierarchic structure" the following is meant: matter is distributed in space so that stars combine to form greater systems, star-systems or galaxies; galaxies again combine to form still greater systems, supergalaxies; and so on. From each rank on the hierarchic ladder we can step to a rank of higher order consisting of elements of the preceding rank, and so forth to infinity. If on such a hierarchic structure we impose certain additional restrictions, the singularities of the above-mentioned character need not appear. In the resulting world the mean density of matter becomes zero.

Let G_0^0 denote a system of lowest rank (the upper index indicates one particular system of this rank); it will be a single star, for instance, our sun.

Let G_1^0 denote the system of the next higher rank, a star-system or a spiral system like the galaxy, containing N_1 stars, among these, in particular, also the element G_0^0, the sun.

Let G_2^0 denote a system of next higher rank, consisting of N_2 spirals, again containing, in particular, the element G_1^0, our galaxy, and so on.

In addition, let us simplify the picture by attributing to all systems a spherical shape, of radius R_0, R_1, R_2, . . . , respectively, each system having the total mass M_0, M_1, M_2, . . . , respectively, and let us finally assume that, in particular, G_0^0 lies on the surface of G_1^0; G_1^0 on the surface of G_2^0. . . . Then M_0^0, the sun, will be subject to a gravitational force derivable from a potential which, on account of the spherical symmetry of every system of any rank i, is equal to the sum of terms M_i/R_i, each term representing the value of the gravitational potential at the surface of a spherical mass of radius R_i and mass M_i. The gravitational potential at G_0^0 is thus given by the infinite series:

$$\frac{M_1}{R_1} + \frac{M_2}{R_2} + \cdots \frac{M_i}{R_i} + \cdots . \qquad (2,2.2)$$

This sum converges if

$$\frac{M_i/R_i}{M_{i-1}/R_{i-1}} = \gamma < 1 , \qquad (2,2.3)$$

i.e., if

$$\frac{R_i}{R_{i-1}} > \frac{M_i}{M_{i-1}} \frac{1}{\gamma}.$$

Now, G_1^0 consists of N_1 stars of mass M_0; hence $M_1 = N_1 M_0$. Similarly,

$$M_2 = N_2 M_1 = N_1 N_2 M_0 ,$$

$$M_3 = N_3 M_2 = N_1 N_2 N_3 M_0 .$$

Consequently,

$$\frac{M_i}{M_{i-1}} = N_i , \qquad (2,2.4)$$

and the potential from which the gravitational force acting upon G_0^0 is derived and which results from a superposition of infinitely many systems in the hierarchic order will be finite if the radii of the various systems satisfy the inequality,

$$\frac{R_i}{R_{i-1}} \geqq \frac{1}{\gamma} N_i \qquad \text{where} \qquad \gamma < 1. \qquad (2,2.5)$$

The special assumptions made here with regard to the spherical shape of each system and its surface position in the next higher system are without influence upon the general conclusions concerning the convergence of the infinite series for the gravitational force.

Olbers' objection is similarly removed by satisfying the same inequality, as is easily shown. This objection is not of the same importance in principle as is Seeliger's objection, for it can also be removed simply by assuming that the universe contains sufficient obscuring dark matter to reduce the integrated light of the sky to the observed value. Thus, in an infinite world consisting of infinitely many bright stars, the total gravitational attraction upon any star (our sun plus the earth, for instance) could remain regular, i.e., have in every point a defined finite value, if the matter were distributed according to the rule outlined above; and also the brightness of the night sky could be kept sufficiently low. In such a universe the mean density, $\bar{\rho}$, of the matter, taken over a volume increasing toward the infinite Euclidean space, converges to zero, although the total amount of matter in the universe would increase beyond all limits when the volume increases toward infinity.

It may be worth mentioning that so far as our present experience goes—it actually covers only the first two steps—a distribution of the stars similar to such a hierarchic structure seems really to be indicated in the universe. The stars of rank 0 combine to form star-systems, spiral nebulae, or star-systems of different structure, of rank 1, and these again tend to agglomerate into systems of the next higher order. The Coma Virgo cluster and other clusters of spiral systems represent such systems of rank 2. This structural feature of the universe has however, not yet found its place in modern cosmological theories. All models of the world still confine themselves to the most simple case of uniform distribution of matter in space and disregard the special features of the actual distribution. It has therefore not yet been carefully investigated how far the ob-

served hierarchic structure in the universe actually would satisfy the special conditions which should insure finite values of the gravitational forces in each volume element of an infinite space.

Another difficulty which was encountered in the first attempts to solve the cosmological problem was the expectation of unduly high velocities in an infinite universe containing an infinite amount of matter. This difficulty is also eliminated in the hierarchic structure. However, at the time when the idea of a hierarchic world order was developed, the high and systematic velocities of distant spiral systems were still unknown and the necessity had not then been realized of conceiving the cosmological problem as a dynamical, and not a static, problem. The universe would be called "static" if—despite local motions of celestial bodies, as, for instance, the orbital motion of the planets around the sun—no large-scale changes in the distribution of matter in the universe were going on. In such a "static" world the mean density in the distribution of matter would remain constant when referred to sufficiently large volumes of space and sufficiently long intervals of time.

When our knowledge extended far beyond the limits of our galaxy and high velocities of the spiral systems were discovered[3] which no longer could be considered small even when compared with the velocity of light, these velocities were of a character very different from the high velocities expected on account of increasingly large potential differences. This discovery, together with the new methods of determining distances in stellar space, extending our "neighborhood" far beyond the limits reached until then, changed the whole outlook on the cosmological problem to such an extent that all previous results had to be completely revised.

At practically the same time the general theory of relativity took up the study of the cosmological problem from a very different point of view, for it had to test its new laws on the cosmological problem.

However, also within the framework of Newton's mechanics, the new empirical facts—in particular, the newly discovered

expansion of the spiral systems—demanded a satisfactory explanation. All empirical facts will therefore be rediscussed from both the classical and the relativistic points of view.

3. The Observational Background for Modern Cosmological Considerations

3,1. The Various Definitions of Distance

The original method of determining the distance of a celestial body is based on the measurement of its trigonometric parallax.

Using as base line the diameter of the earth's orbit, periodic
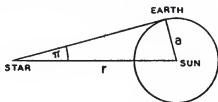
changes in a star's apparent position due to the orbital motion of the earth in the course of a year, when measurable, enable us to derive its distance. The mean radius of the earth's orbit is $a = 1.495 \cdot 10^{13}$ cm. The parallax, π, of a star, defined as the angle subtended by the radius of the earth's orbit as seen from the star (see Fig. 1), is thus connected with the distance r by the equation

$$\sin \pi = \frac{a}{r}. \tag{3,1.1}$$

Since π is always a very small angle, smaller than $1''$, the preceding formula may be written as follows:

$$\pi'' \sin 1'' = \frac{a}{r},$$

when π'' denotes the value of the parallax in seconds of arc; hence

$$\pi'' = \frac{a}{r} \frac{1}{\sin 1''} = 206265 \times \frac{a}{r}. \tag{3,1.2}$$

The distance r corresponding to a value of $\pi = 1''$ has been

chosen as a convenient unit of distance and is called 1 parsec; its numerical value is $3.084 \cdot 10^{18}$ cm $= 3.26$ light years.

Reliable measurements of trigonometric parallaxes are not possible for values of π far below $0.01''$; thus distances beyond 100 parsecs are not within the range of measurable trigonometric parallaxes. It is chiefly due to this fact that all cosmological considerations before, let us say, 1920, to which the preceding section refers, have been of a rather academic character. It is now obvious that only when our knowledge extends to distances of the order of millions of parsecs can we hope to draw conclusions concerning the structure of the universe at large, for instance, conclusions for or against a finite or infinite world.

Thus cosmology experienced its revival in modern times when the great break-through to star-systems far beyond the limits of the galaxy succeeded. This advance from distances of the order of 10^3 parsecs to distances of the order of 10^8 parsecs became possible when methods of determining distances in interstellar space were developed which were independent of a direct reference to a base line. For the limitation of the method of measuring trigonometric parallaxes is due to the extreme smallness of the radius a of the earth's orbit as compared with the average distances of stars in our galaxy, not to mention its smallness relative to distances of extragalactic star-systems.

The emancipation from a direct reference to a base line succeeded when a more detailed astrophysical knowledge of various types of stars was obtained. This knowledge made it possible to reverse in special cases the equation,

$$m - M = 5 \log r - 5 , \qquad (3,1.3)$$

which connects the absolute magnitude M of a star, i.e., its luminosity (see note 1), with its apparent magnitude, m, and its distance, r, that is, not to use this equation to determine M but to derive from it the value of r. Originally, this equation was formulated to define the luminosity of a star in terms of magnitude classes. The apparent brightness, m, of a star depends on two factors, namely, the luminosity of the star, i.e., the radia-

tion emitted per second from its surface, and its distance. A very bright star may appear very faint, i.e. its m is large (the value for the magnitude class increases with decreasing brightness, a star of first magnitude being very bright), if it is situated at a sufficiently great distance from the earth. Consequently, M can be derived only when the distance r of the star, i.e., its parallax, has been measured. Then, if we define M as the star's apparent magnitude at the standard distance of 10 parsecs and suppose space to be absolutely transparent, M can be determined from equation (3,1.3) if r is known. But the process can be reversed, and the value of $(m - M)$ can be derived, if independent means have become available to determine the value of M, for a star or for a whole star-system. Then the distance r can be calculated from our equation; for this reason $(m - M)$ is called the "distance-modulus."

Distances obtained in this way may be called "photometric" distances; for they depend on the knowledge of the absolute magnitude M, i.e., of the luminosity of the celestial body under consideration. No special base line limits the possibility of deriving distances from the distance-modulus, as in the case of trigonometric parallaxes.

The radius of the earth's orbit, the fundamental base line for measuring star parallaxes, is, of course, implicitly involved; for any independent statement on the absolute magnitude M of a star from purely astrophysical data is possible only with the help of stars of which the parallaxes are known.

The base line a enters thus into the calibration of the rule that discloses the star's luminosities. When this rule has been established, a star needs only to be identified as a member of the special class to which the rule refers, and its distance results from equation (3,1.3).

With the general widening of our astrophysical knowledge, various rules were discovered yielding the calculation of luminosities of special stars or whole star-systems from special astrophysical characteristics. As far as individual stars are concerned, such information refers to the absolute magnitude and the periods of Cepheid variables, or to the upper limit of

luminosity reached by novae, or to the upper limit of luminosity observed for normal stars.

For Cepheid variables a relation between the period of their variability and their luminosity was discovered in 1912 by a study of the numerous Cepheids found in the Magellanic star clouds. This period-luminosity relation has been carefully calibrated by the use of Cepheids of measured distances within our galaxy. After that was done, it was only necessary to measure the period of light change for any Cepheid, observed anywhere in space, then to read the corresponding value of M from a graph giving the relation between period and luminosity and finally to find r from equation (3,1.3).

As to novae, observations have disclosed that in the phase of greatest brightness the absolute magnitude scatters only moderately around a mean value near to $M = -5.7$. So, whenever in a distant star-system the flashing-up of a normal nova (not a supernova, which belongs to a different class of objects) is observed near its maximum of brightness and when the apparent brightness m in this phase has been measured, the value of $(m - M)$ may be estimated with reasonable certainty, and thus also the distance r. Here again the "base line," i.e., the radius of the earth's orbit, enters by means of the calibration from which the approximately constant value of the luminosity in the maximal phase for various novae had been derived.

Third, statistical investigations of the absolute magnitudes of stars of known parallaxes in the galaxy revealed the fact that for normal stars a well-defined upper limit of M exists. The mean absolute magnitude of, say, the 25 brightest stars observed in a distant star-cluster may therefore be supposed to fall close to this upper limit; this again provides an estimated value of $(m - M)$, and so also of r.

By applying such methods, the distances of globular star-clusters and various other star-systems were determined, and the range of measurable distances was extended from a few hundred parsecs to millions of parsecs. It was recognized that the globular clusters, although members of our galaxy, are situated outside its main body at distances ranging from 6 to

50 kiloparsecs (1 kiloparsec = 10^3 parsecs). A still deeper penetration into space by similar methods brought the wealth of extragalactic star-systems within reach at about 300 kiloparsecs, and valuable knowledge was obtained concerning the next higher class of systems in the hierarchy of world structure. Only very few of these extragalactic systems are near enough to allow observations of individual stars belonging to them. A further extension of our knowledge to still greater distances had to be based on methods specially developed from a study of their apparent sizes, from the internal structure and integrated luminosity of extragalactic systems, etc. It was found that the absolute magnitudes, M, of whole star-systems similar in structure—spiral systems or globular clusters—scatter only moderately around a mean value. This mean value of M can therefore be used to estimate with sufficient accuracy the distances of spiral systems or globular clusters from the distance-modulus, in which m and M now denote the apparent and absolute magnitudes of a whole star-system. It is true, distances derived in this way locate a star-system only with a certain statistical probability as at a certain distance; they are not necessarily individually accurate. Similarly, the apparent diameters may be used to derive what are called "spatial distances," based on the fact that the linear dimensions of systems of similar structure appear to scatter only moderately around a mean value.

The next step was to correlate distances of star-systems with the observed values of red-shifts of the lines in the spectra of distant star-systems.

A general red-shift of the lines in a spectrum may result from a recession of the light source, star or star-system, from which the spectrum is produced. In this case the speed of recession dr/dt, r denoting the distance of the light source, is related to the red-shift, i.e., the observed increase of the wave length λ of the spectral lines, by the equation

$$\frac{dr}{dt} = c \frac{d\lambda}{\lambda}, \qquad (3,1.4)$$

where c is the velocity of light.

The correlation between the observed red-shifts of spiral systems and their distances yielded the equation

$$\frac{dr}{dt} = \kappa r, \tag{3,1.5}$$

where κ is a constant; this is the law of the general expansion of the universe.

The formula (3,1.5) calibrated for the nearer systems with known distances, furnished a further possibility of estimating distances when only observed red-shifts are available. In this way our knowledge concerning the distribution of matter in space was widened step by step through a great variety of methods of measuring or estimating distances.

When the problem of cosmology was revived in connection with the theory of relativity, the surveyed part of the world extended to distances of nearly 10^8 parsecs, containing in its volume about one hundred million star-systems, each consisting of some thousand million stars. This was an empirical background very different from that on which astronomy had had to rely when it first attempted to tackle this complex problem. The space surveyed is no longer so insignificantly small as to make all considerations purely speculative. In particular, the newly discovered red-shifts, interpreted as an expansion of the universe, demanded a solution of a character very different from the static world (see p. 8) which formerly was taken for granted.

3,2. Remarks on the General Red-Shift

The correct interpretation of the red-shift is the key to a full understanding of the cosmological problem. Strong doubts about the right of interpreting the observed red-shifts of extragalactic star-systems as Doppler effects have been expressed. These doubts originate chiefly in the desire to keep the door open for a static world, according to which the world as a whole may be understood as being at rest, in equilibrium, and not itself involved in a gigantic evolution.

Are there any sound reasons to justify such doubts? During

the past century the measurement of shifts of spectral lines and their interpretation as resulting from velocities in the line of sight has proved to be one of the most powerful and never failing tools of astrophysics. Neither within the range of globular star-clusters, that is, up to distances of 10^4 to 10^5 parsecs, nor within the range of the nearest spiral systems, from 3.10^5 to 5.10^5 parsecs, do the observed displacements of the spectral lines in their spectra give any reason for suspecting that these shifts of spectral lines may not be interpreted as Doppler effects. Positive and negative values are measured. Beyond a certain distance, it is true, red-shifts, i.e., positive values for $d\lambda/\lambda$, prevail, indicating a general recession of the more distant spiral systems, systems which definitely no longer belong to the cluster of star-systems of which our galaxy appears to be a member. A general expansion of the world is indicated. On the other hand, as we shall see, theoretical considerations show that no solution of the cosmological problem is to be expected yielding a static world in which gravitating matter fills the universe with a finite value of the density. A world at rest, filled with gravitating matter, is not possible. So observational facts indicating systematic motions of the world matter and changes in its density distribution had to be expected. The only surprising fact might perhaps be that the observations disclose such a simple change, which in the first approximation might be described as a universal, regular, isotropic expansion of the universe. Nothing yet justifies the assumption that in the red-shift of the spectral lines of extragalactic systems we are dealing with a fundamentally new and hitherto unknown phenomenon of light emission.

It is naturally possible that in the future we may have to change this view under the weight of new discoveries. However, until these come forward, we should follow the route that so far has not brought us to a wrong turn and interpret the red-shifts as Doppler effects.

The numerical value of the constant factor κ in the formula (3,1.5) which expresses the expansion of the world, is near to 500 km/sec, when r is measured in units of 10^6 parsecs.

Spiral systems at about 1,000,000 parsecs distance recede with a speed of about 500 km/sec; at 10^8 parsecs for the most distant spiral systems, so far observed, the speed of recession is already of the order of one-sixth of the velocity of light. Such unexpectedly large values for the velocities, hitherto never reached in nature unless for electrons or particles of atomic size, have naturally intensified the doubts as to whether we are entitled to interpret the red-shifts of star-systems as Doppler effects at all.

3,3. The Apparent Distribution of Matter in Space

So far the survey of star-systems indicates, if the facts in favor of a hierarchic structure are disregarded, a uniform distribution of matter in space in accordance with formula (2,1.1), i.e. $\log N_m = 0.6 \cdot m + \text{const.}$, where N_m is now the number of nebulae brighter than the limiting magnitude m. If the unit of volume is taken sufficiently large to smooth out local irregularities in the distribution, the resulting number of star-systems per unit volume is roughly constant at all distances and in all directions. Spreading the matter, concentrated in the stars, evenly over the whole space, a constant value ρ for the density of matter is obtained. Accordingly, the constant value for ρ will be assumed in all following considerations.

This result, however, is not independent of the interpretation of the observed red-shifts $d\lambda/\lambda$; for the values of m in formula (2,1.1) are systematically influenced by the increase of the values of $d\lambda$ with increasing distances. The apparent brightness, m, of a nebula is in two respects affected by a red-shift. The quantity m measures the energy, i.e., the number of light quanta, received from the light source in question within a certain range of wave lengths per unit of time and unit of receiving surface. The energy E of a light quantum is given by $E = h\nu$, where ν denotes the frequency and h is Planck's constant. Since $c = \lambda\nu$, and the light velocity is constant, $E\lambda = hc$ is also constant. Therefore, if E^* measures the energy of the quantum affected by a change of wave length equal to $\Delta\lambda$, the product $E^*(\lambda + \Delta\lambda)$ must be equal to $E\lambda$; hence

$$E = E^* \left(1 + \frac{\Delta\lambda}{\lambda}\right). \tag{3.3.1}$$

Thus, if $\Delta\lambda$ is positive, the energy of the quantum is reduced by the factor $[1 + (\Delta\lambda/\lambda)]$. This reduction of the energy, and thus also of m, must be considered, whatever physical phenomenon produces the observed shift, as long as the equation $E = h\nu$ is maintained.

The total amount of this correction, when all further selective influences, like that of the earth's atmosphere, light losses in the telescope, etc., on the bolometric luminosity of the star-systems are included, is about $3\Delta\lambda/\lambda$. The Bootes group of spirals, for instance, for which $\Delta\lambda/\lambda = 0.131$, thus calls for a correction of about 0.39 magnitude, by no means an insignificant correction.

When this correction is applied to the observed m of the star-systems, we obtain an apparently uniform distribution of matter in space, in agreement with the formula: $\log N_m = 0.6 \cdot m$ + constant.[4]

When, however, the red-shift is interpreted as a Doppler effect, an additional correction, amounting to an additional term $\Delta\lambda/\lambda$, has to be considered. For now one has to account for the additional fact that the brightness of a receding light source is reduced because, owing to its speed of recession, less quanta reach the observer's eye or the photographic plate, per unit of time, than would reach it if the light source remained at the same distance. The application of this recession correction changes the relation between m and $\log N_m$. There is no longer an agreement with the assumption of a uniform distribution of matter in space. The significance of this fact will be discussed further on.

4. The Cosmological Postulate

4,1. The Nature of the Postulate

Although, as has just been pointed out, observations do not unconditionally agree with the hypothesis of an expanding universe filled with matter of a constant mean density ρ, the cos-

mological models of the world have hitherto been developed on this assumption. Otherwise the problem would still defy all our efforts to find a solution. Do the laws of nature give a satisfactory description of such an expanding world, and are the available data sufficient to decide whether the world is finite or infinite? In what respects does the relativistic approach to the problem yield more satisfactory results? These are the questions to which we wish to give an answer.

We shall approach these questions by making the postulate that the observed phenomena are representative of the whole universe; only then will the results represent a cosmological theory. This cosmological postulate will be:

Every observer, wherever placed in the universe, describes the observed phenomena identically, saying that he is placed in an isotropically expanding universe in which matter is evenly distributed.

This postulate brings to conclusion a development of science which had its beginning with Copernicus and Kepler. In much the same way as they removed the earth, and thus man as well, from the central position which man in those days claimed for himself in his restricted picture of the world, the cosmological postulate deprives the picture of the whole universe of all features that tend to grant to man any privileged central position.

4,2. The Theory of an Expanding Universe on Classical Grounds

When, about thirty years ago, the attempts to solve the cosmological problem were resumed, the point of departure was very different from that of former attempts.

The space actually surveyed is no longer a merely infinitesimal part of the star-system to which the solar system belongs. It comprises many more star-systems than was the number of single stars included in the earlier surveys. We know that a stationary world in which matter may, in the first approximation, be considered as being at rest is, on principle, not possible —apart from a very special case (see p. 52)—and we know also that actually such a world is not indicated by observations.

All this rightly raised hopes that a resumption of the cosmo-

logical problem would bring us nearer to a solution than earlier attempts could have hoped to achieve. Will a decisive advance towards this solution come from the vast extention of our knowledge of the distribution of matter in space, including the phenomenon of expansion, or from the development in our knowledge of the principles of physics and geometry, stimulated by the theory of relativity? It is worth while to resume once more the cosmological considerations on classical, nonrelativistic grounds, in order to answer this question.

We consider matter, condensed in star-systems, as uniformly distributed in space and treat it as a continuous substratum filling space like the molecules of a fluid. Neglecting, first, the gravitational forces, we ask: Which form of motion of this matter in space is compatible with the cosmological postulate? Is, in particular, an isotropic expansion of the spiral systems, derived from the general red-shifts as represented by the formula $dr/dt = r \cdot \text{constant}$, compatible with the postulate that every observer, wherever placed in the universe, should form the same picture of the world? A more exact mathematical discussion will be given in the supplement to this section; here only the general trend of the ideas and results will be outlined.

Every observer considers himself at rest while the outside world is moving relative to his position in a very special way, namely, receding in all directions symmetrically, with speeds increasing proportionately to the increase of distance. No rotation of the world relative to any position is observed. If an observer 1 attributes to the universal substratum at an arbitrary place P_1, characterized by space coordinates, x_1, x_2, x_3, a certain density $\rho(x_i, t)$ at a moment t, any other observer 2, receding from 1 but considering himself again at rest in his own world, must attribute to the substratum at the corresponding place, i.e., the place located relative to his position, as P_1 was located relative to observer 1—a position characterized now by coordinates x'_1, x'_2, x'_3—the same density $\rho(x'_i, t)$ at the same moment. This must be true for any position whatsoever. Obviously, the density ρ of the substratum cannot be a function of the spatial coordinates x_i at all, but only of the time t.

The substratum must consequently fill space with uniform

density ρ; the value of ρ may, however, change with time, provided that at every moment this change is the same at every place. A change of just this character is produced by an isotropic expansion, during which the amount of matter, supposed to be invariant within every finite volume of space, continues filling with uniform density the steadily increasing volume of space allotted to it. Thus we find, from kinematical considerations only, that the conception of a uniformly expanding (or also contracting) universe, filled with matter of constant density (the density being a function of time only), introduces, in principle, no new difficulties into the cosmological problem.

The difficulties which were encountered in earlier attempts to solve this problem originated in the gravitational field set up by the world matter, if space was supposed to be of infinite volume and filled in its entirety with matter. It was found impossible to define the gravitational potential in a satisfactory way when a static solution of an infinite world was envisaged.

Now the situation is changed for two reasons; the general expansion of the universe demands the solution of a very definite dynamical problem, namely, the dynamical explanation of the expansion from universal gravitation; the cosmological postulate, on the other hand, reduces strongly the possible variety of solutions. For the general expansion only the gravitational field produced by the world substratum can be made responsible. This makes it necessary to define clearly the potential arising from this field at every point, and in doing this some of the uncertainties which formerly hampered the progress of the whole problem are avoided. The general field originating from the evenly distributed expanding world substratum will be called the "inertial field," to indicate that this field—in accordance with the principle of equivalence in the theory of relativity—may be made responsible for the appearance of the inert mass of bodies but not for local orbital motions as they are observed, for instance, in the neighborhood of the sun (see also p. 52).

Owing to the existence of this inertial field, no observer in the world should experience any accelerations which would make it

impossible for him to consider himself at rest in his world. Therefore, if ϕ denotes the value of the potential at an arbitrary point in a system S, characterized by the coordinates x_1, x_2, x_3, relative to the observer's position in the center, and $\partial\phi/\partial x_1$, $\partial\phi/\partial x_2$, $\partial\phi/\partial x_3$, the components of the force derived from this potential, these components must vanish whenever $x_1 = x_2 = x_3 = 0$, that means, at the observer's place; and this must be true for every imaginable system S connected with an observer. The mathematical expression for ϕ must consequently have terms of second order as terms of lowest order in the space coordinates, so that, when $x_i = 0$, $i = 1, 2, 3$, all three components $\partial\phi/\partial x_i$ vanish.

In view of the symmetrical character of the distribution of matter, it is natural to postulate for ϕ an expression of the special form $\phi = A(x_1^2 + x_2^2 + x_3^2)$, where A may be a function of the time t. If, for instance, we take A equal to $\frac{2}{3}\pi\kappa_0\rho(t)$, we would obtain

$$\frac{\partial\phi}{\partial r} = \frac{\kappa M(r)}{r^2}, \qquad (4,2.1)$$

where $M(r)$ denotes the mass inclosed in a sphere of radius r. With such an expression for ϕ, the difference of the value of the potential at any two points P_1 and P_2 depends on their relative distance only. Therefore, taking P_1 as the center of a system S and describing a sphere around P_1, passing through P_2, the value of ϕ will be the same at all points on this sphere, and every point may be considered as screened off from the world outside this sphere. A similar statement can be made if P_2 is taken as the observer's place, i.e., as the center of the system, and P_1 is located on the sphere around P_2. The expansion of the universe in the field defined by this potential ϕ becomes a radial, rectilinear motion of P_2 relative to P_1 (P_2 being quite an arbitrary point), describable by the equation of motion

$$\frac{d^2r}{dt^2} = \frac{\partial\phi}{\partial r},$$

which, if $M(r)$ again denotes the mass inclosed in the sphere of radius r and k_0 the gravitational constant, takes the form:

$$\frac{d^2 r}{dt^2} = \frac{-\kappa_0 M\,(r)}{r^2}. \tag{4,2.2}$$

To r we may give the special form: $r = r_0 R(t)$, thus attributing the isotropic expansion to the change in time of a general scale factor R, which is a function of t, while r_0 denotes the arbitrary distance of P_2 from P_1 at an arbitrarily chosen moment $t = 0$. The last equation then becomes independent of the value r_0, i.e., independent of the special choice of P_1 and P_2, and reads:

$$\frac{d^2 R}{dt^2} = \frac{-\kappa_0 M}{R^2}, \tag{4,2.3}$$

where M is a constant, stating that in an expanding volume the inclosed mass remains constant.

The dynamical problem inherent in the expansion of the universe is thus formally reduced to a very simple problem of celestial mechanics, namely, that of the expansion of a spherical system of bodies in which every body moves radially and the value of the density ρ remains constant throughout any volume, the numerical value of ρ naturally decreasing with increasing volume.

The dynamical problem to which we have reduced the expansion is easily solved. The character of the solution depends on the value and sign of the constant of integration h, which enters when a first integration is performed. This yields

$$\tfrac{1}{2}\dot{R}^2 = \frac{\kappa_0 M}{R} + h, \tag{4,2.4}$$

where $\dot{R} = dR/dt$ measures the speed of expansion. We have expanding universes of a periodic character when the constant h is negative. In this case the expansion should come, in a finite time, to a standstill and reverse into a contraction, which, after a finite time, should lead to a singularity when for $R = 0$ the density ρ becomes infinitely large, since $M = \tfrac{4}{3}\pi\rho R^3$ is a constant.

This case, $h < 0$, is called in celestial mechanics the "elliptic" case. The parabolic and hyperbolic cases correspond to $h = 0$ and $h > 0$, respectively. They would not predict any catastrophic singularity of the universe in the future, for in both cases R will steadily increase to infinity. But the expansion must

have started from a singular initial state, if we follow the expansion backward until R becomes zero. The dynamical problem of the universal expansion introduces in every case a catastrophic beginning or ending of the world.

In this model of an expanding universe the transition to infinity, i.e., to a space of infinite volume, has to be done by exhausting the space with an infinite sequence of concentric spherical shells, all centered around the observer's place. The value of the potential, being proportional to the square of the radius of each shell, increases then in a definite way beyond all limits if the mean density of matter, taken over the finally infinite volume of space, is supposed to remain finite. Granted that, owing to the assumed absolute symmetry in the distribution of the substratum, no observer suffers an acceleration, the gravitational pull or "tear" would nevertheless become undetermined when an infinite world is envisaged.

This difficulty could be evaded by changing the assumption from which we started and considering the world as having a hierarchic structure as discussed in section 2. A finite regular value of the potential in the limit of an infinite volume of space can then be guaranteed. The hierarchic model which we had discussed formerly would have to be complemented only by attributing to it a general expansion.

The condition for regularity had been (2,2.5)

$$\frac{R_i}{R_{i-1}} \geqq \frac{1}{\gamma} N_i.$$

This inequality must remain valid for all times.

It is easily seen that, for instance, a speed of expansion proportional to the radius R_i of the system of rank i would satisfy this condition.

In such an expanding universe of hierarchic structure the limiting value of the potential would remain finite, and there would be no reason for giving up Newton's law of gravitation. But an expanding and contracting world introduces new singularities of a different character. They occur when the density tends to infinite values; and such singularities have to be en-

visaged also in an expanding world of hierarchic structure when Newton's law of gravitation is retained.

Various changes in the formulations of this law were considered when the cosmological considerations were still aiming at a static solution of the problem. One had considered, for instance, the possibility of a faint absorption of gravitation in space which would extinguish the gravitational effect of the infinitely distant masses. Such a correction of Newton's law would not remove the kind of singularity that appears in an expanding world. Another way to remove these singularities would be to supplement the expression for the potential $\phi = k[M(r)/r]$ in Poisson's equations by a term λr^2, which means introducing a repulsive force in addition to gravitation. The constant value of λ (λ has nothing to do with the wave length λ in the preceding section) would have to be adjusted to the actual conditions in the universe, so that for small parts of the universe (which, however, could still cover practically all the world so far explored) the field of the repulsive force would be very weak compared to the gravitational field of the world substratum.

This special correction will be discussed in more detail because it removes the singularities and because in the later relativistic treatment of the cosmological problem quite similar singularities are met by introducing a similar correcting term.

When the effect of such a cosmic repulsive force is considered, new solutions of the dynamical problem result. A contracting world may be brought to a standstill before R reaches the value zero and before ρ becomes infinite. For this purpose the constant h must be chosen < 0, while λ has to be > 0, and the value of M must be chosen appropriately.

If the actual universe corresponded to such a solution, we would have to assume that the expansion, which we observe at present, was preceded by a contraction which came to a standstill at regular values of R and ρ, respectively. When this happened, the tendency to contract was counterbalanced by the tendency to expand due to the repulsive force. After that the universe began to expand again, and we are experiencing this phase at present.

Such a solution of the cosmological problem avoids the occurrence of catastrophic singularities in the life of our universe. It must, however, be realized that this is achieved only by the *ad hoc* hypothesis of a cosmic repulsive force, for which, so far, no other proof or indication exists.

The classical treatment does not lead to a deeper understanding of the cosmological problem; in particular, no insight into the question is gained as to whether the universe might be finite or infinite. In the relativistic treatment, as we shall see, the equivalent correcting term, called the "cosmological constant," opens quite novel aspects of this fundamental question.

The uncertainty with regard to the possible finiteness of space could be eluded simply by attributing to space the metric properties of a finite, closed, non-Euclidean space. However, since the classical theory does not establish any link between geometry and gravitation, this would represent a further *ad hoc* hypothesis, which could be neither proved nor disproved, unless our empirical knowledge of the structure of the universe could be extended to distances not small compared with the radius of curvature of space. However, a priori no possibility exists of predicting the value of this radius of curvature. And even should experience prove the correctness of such a hypothesis, our understanding of the origin of the metric properties of space would not have been advanced.

4,3. Summary

Therefore, in summarizing the results so far obtained, we can state:

The tremendous extension of our knowledge of the distribution of matter in the universe has, as far as an advancement of the cosmological problem within the framework of classical physics is concerned, not brought us much nearer to a solution of the problem. The general expansion of the universe which has now to be considered and which demands a dynamical solution of the cosmological problem does not produce new complications in principle. On the contrary, it leads to an unambiguous definition of the potential function to satisfy the cosmological

postulate and to describe dynamically the expansion; thus uncertainties which formerly remained are avoided. The expansion can be easily incorporated and explained on the ground of known problems in celestial mechanics. As a new feature, singularities appear which make it impossible to apply the dynamical solution for the expansion to the whole lifetime of the universe (see later, sec. 6,1).

The one difficulty of the cosmological problem, i.e., the fact that Newton's law of gravitation combined with the conception of an infinite Euclidean space, filled in its whole extent with matter of a finite average density, can be removed by assuming a hierarchic structure of the universe.

Indications of such a hierarchic structure actually exist. However, the survey of spiral systems does not contradict, so far, the assumption of a uniform distribution of matter, if the scale is chosen sufficiently large. This most simple assumption of uniform distribution has, therefore, until now been preferred for the model of the universe.

The hierarchic structure can be chosen so that the potential at every place remains regular when the transition to one finite space is performed, and Newton's law of gravitation may be retained without restriction for the whole universe. Also the phenomenon of expansion could easily be incorporated into the picture of a universe of hierarchic structure. But singularities, characteristic of the dynamical problem which describes the expansion, necessarily appear; and they can be removed only by a change of Poisson's fundamental equation for the potential. This means in every case an abandonment of Newton's law. Various amendments of Newton's law offer themselves. One is to allow for a repulsive force apart from gravitation, this repulsive force becoming effective only in dimensions large compared with the space so far explored, so that all local orbital motions of the whole galaxy are mainly ruled by gravitation. This amendment also permits the removal of the singularities arising from a catastrophic crash (the initial explosion of the universe) and offers in addition a solution in which matter is, on the average,

uniformly distributed through space. No special hierarchic structure need be further envisaged.

Such amendments would in every case be based on an *ad hoc* hypothesis, valid only for the special purpose of offering an acceptable solution of the cosmological problem free from singularities within the framework of Newton's mechanics. They do not essentially enrich our understanding of the whole problem. In the relativistic treatment which follows, similar amendments have also to be made *ad hoc;* but, by opening new aspects of the greatest importance, they obtain special significance.

The inability of Newton's theory to relate the forces in nature to the metric properties of space deprives it of the possibility of bringing the fundamental question of cosmology: "Is the universe finite or infinite?" within the reach of cosmological considerations.

4,4. Supplement

A. The infinite Euclidean space is filled uniformly by a "substratum" of spiral nebulae, which will be treated like the molecules of a gas distributed uniformly through space.

Let x_1, x_2, x_3, be the three Cartesian coordinates of a point referred to the observer's position in system S, and let x_1', x_2', x_3', be its coordinates with reference to another system S', supposed to be in motion relative to S. The equations connecting the coordinates in S and S' are $x_i = a_i + x_i'$, where the a_i, functions of time, define the relative position of the observers in the systems S and S', respectively. The streaming of matter at a point (x_i) is described by velocity components v_i:

$$v_i(x, t) = \frac{d a_i}{dt} + v_i'(x', t), \qquad i = 1, 2, 3 . \ (a)$$

Every observer, placed at the zero point of his system of reference, S or S', considers himself at rest with regard to his immediate neighborhood. Consequently, $v_i(x, t) \equiv 0$ when all three coordinates $x_i = 0$, and equally $v_i'(x', t) \equiv 0$ when all $x' = 0$.

According to the cosmological postulate, each observer arrives at the same description of the world surrounding him. The

density ρ and pressure p ascribed to the substratum at any point P_1 by an observer in S must be equal to the corresponding values ρ' and p' ascribed at the same moment by an observer in S' to the substratum at a point P_1' having, relative to the observer in S', the same coordinates as P relative to the observer in S. That means $\rho(x_i, t) \equiv \rho'(x_i', t)$ when $x_i = x_i'$. Similar equations hold for p and v_i. Since the values of the coordinates x_i may be arbitrarily chosen, it follows that $\rho = \rho(t)$ and $p = p(t)$, i.e., ρ and p are functions of the time t only. The velocity components v_i may still depend on the space coordinates x_i; for they are defined only relative to an arbitrarily chosen point and do not have the character of local properties of space.

It can easily be shown that the v_i must be linear functions of the space coordinates.

If in equation (a), we put $x_i' = 0$, we find

$$v_i(a_i, t) = \frac{d a_i}{dt};$$

and, since $x_i' = x_i - a_i$, this leads to the functional equations

$$v_i(x_i - a_i, t) = v_i(x_i, t) - v_i(a_i, t),$$

which must be satisfied for any value of a_i. This implies

$$v_i = a_{i1}x_1 + a_{i2}x_2 + a_{i3}x_3, \qquad i = 1, 2, 3 \quad (b)$$

where the nine functions a_{ik} are functions of time only.

A world expanding (or contracting) symmetrically in all directions corresponds to the special case $a_{11} = a_{22} = a_{33} = f(t)$, $a_{ik} = 0$, when $i \neq k$. If we set $x_i = x_i^0 R(t)$, where $R(t)$ is a function of t only and represents a universal scale factor, while the x_i^0 are arbitrary initial values of the coordinates of any point in a system S, the expansion (or contraction) is described by the equations:

$$v_i = \frac{d x_i}{dt} = x_i^0 \frac{dR}{dt} = x_i \frac{\dot R}{R}, \qquad \text{since } x_i^0 = \frac{x_i}{R(t)}; \qquad \left(\dot R = \frac{dR}{dt}\right).$$

We thus obtain a law of expansion corresponding to that derived from the observed red-shift of the spiral systems.

So far the considerations have been of a purely kinematical

nature. They have not taken account of the fact that all matter in the world is gravitating and, when moving, transports momentum. Conditions therefore have to be added which make sure that (1) all matter is preserved, i.e., no matter is lost, unless transformed into radiation; this last possibility will be, however, disregarded; thus the law of continuity must be satisfied; (2) the preservation of momentum is assured as well; (3) finally, an assumption about the law of gravitation has to be made; it will be here for simplicity assumed that matter gravitates according to Newton's law.

The following equations have thus to be satisfied:

$$\frac{\partial \rho}{\partial t} + \frac{\partial}{\partial x_i} (\rho v_i) = 0 , \qquad \text{i.e., equation of continuity;} \quad (4,4.1)$$

$$\frac{\partial}{\partial t} (\rho v_i) = -\frac{\partial}{\partial x_k} (\rho v_i v_k) - \rho \frac{\partial \phi}{\partial x_i}, \qquad \begin{array}{c}\text{conservation of} \\ \text{momentum;}\end{array} \quad (4,4.2)$$

$$\frac{\partial^2 \phi}{\partial x_1^2} + \frac{\partial^2 \phi}{\partial x_2^2} + \frac{\partial^2 \phi}{\partial x_3^2} = 4\pi \kappa_0 \rho , \qquad \begin{array}{c}\text{Poisson's equation} \\ (\kappa_0 = \text{gravitational constant}).\end{array} \quad (4,4.3)$$

In these equations summation from 1 to 3 has to be performed for every index appearing twice in the same term. In equation $(4,4.2)$ no term depending on a hydrostatic pressure p enters, for, since p has been assumed to be independent of the space coordinates, the term $\partial p/\partial x_i$ which otherwise would appear drops out. The first term on the right-hand side of equation $(4,4.2)$ takes into account a possible influx of momentum due to streaming. About ϕ, the gravitational potential, special assumptions will be made shortly.

If we restrict the considerations to the case of an isotropic expansion, only the diagonal terms of the matrix (a_{ik}) are $\neq 0$; so $v_i = a_i x_i$, and equation $(4,4.1)$ yields

$$\rho + \rho (a_{11} + a_{22} + a_{33}) = 0 , \qquad (4,4.4)$$

while equation $(4,4.2)$, which, owing to formula $(4,4.1)$, may be written in the form

$$\frac{\partial v_i}{\partial t} + v_k \frac{\partial v_i}{\partial x_k} = -\frac{\partial \phi}{\partial x_i},$$

yields

$$\dot{a}_{ik} x_k + a_{ik} a_{ks} x_s = \frac{\partial \phi}{\partial x_i}, \qquad (4,4.5)$$

where, on the left-hand side, only the terms enter for which $i = k$. Hence

$$- \left[\frac{\partial^2 \phi}{\partial x_1^2} + \frac{\partial^2 \phi}{\partial x_2^2} + \frac{\partial^2 \phi}{\partial x_3^2} \right] = \dot{a}_{11} + \dot{a}_{22} + \dot{a}_{33} + \Sigma i \, a_{ii}^2 = - 4\pi \kappa_0 \rho \, ;$$

and this, introduced into the differential equation of continuity,

$$\frac{d}{dt} \left(\frac{\dot{\rho}}{\rho} \right) = - \left(\dot{a}_{11} + \dot{a}_{22} + \dot{a}_{33} \right),$$

gives

$$\frac{d}{dt} \left(\frac{\dot{\rho}}{\rho} \right) = 4\pi \kappa_0 \rho + \Sigma i \, a_{ii}^2, \quad \text{which is} \quad \geqq 0 \, . \quad (4,4.6)$$

This equation is of great and general significance. It is incompatible with the assumption of a static world with $\rho = \text{constant}$ > 0; for $\dot{\rho} = 0$ and $a_{ii} = 0$ entails $\rho \equiv 0$. It is thus impossible to imagine a world in which gravitating matter remains at rest; it is also impossible to let matter stream in such a way that the density remains constant.

B. The Gravitational Potential

According to the cosmological postulate, every observer placed at the origin of an admissible system S considers himself at rest relative to his immediate neighborhood. Hence the field characterized by the potential ϕ must not give rise to any forces tending to displace the observer. It must be realized that we are only considering the "inertial" field arising from the uniformly distributed world substratum which is responsible for the movements of any free particles. The study of local orbital motions at any special place—for instance, in the immediate neighborhood of the sun or within a star-system—owing to a special distribution of the gravitating matter is not considered. The potential ϕ must, in consequence, have such a form that its derivatives, $\partial \phi / \partial x_i$, $(i = 1, 2, 3)$, vanish for $x_i = 0$, thus producing no resultant force; i.e., the representation of ϕ by power series in x_i must begin with terms of at least the second order. We shall choose ϕ as a quadratic form of the coordinates—in particular, in view of the isotropic character of the field, as

$$\phi \, (x_i) = A \, (t) \, (x_1^2 + x_2^2 + x_3^2) \, . \quad (4,4.7)$$

Then the potential difference between two points P_1 and P_2 will depend only on their relative distance. Equation (4,4.3) yields $A = \frac{2}{3}\pi\kappa_0\rho(t)$, if only the field arising from gravitating influence of the matter inside a sphere around P_1 reaching to P_2 is considered. However, it must be understood that, in general, a further term λr^2 with an arbitrary factor λ may be added. The field arising from such a term will be considered later. From the special formulation of the potential it follows that its value at an arbitrary point P_2 depends only on its distance from the origin P_1 of the system. Therefore, if we produce a sphere around P_1 reaching to P_2, each point on the sphere may be regarded as screened off from all gravitational influence of the masses lying outside the sphere.

C. The Differential Equation for the Scale Factor $R(t)$

It is obvious that our description of the phenomenon of the universal expansion (or contraction) must be independent of the choice of the distance $r = r_0 R(t)$ of P_2. We must be able to describe it in terms of the changes of the scale factor $R(t)$ only. While the volume of a sphere contracts or expands, the density ρ of matter contained in it changes continuously but has the same value at all points inside.

Introducing the value $r = r_0 R(t)$, into the differential equation

$$\frac{d^2 r}{dt^2} = \frac{-\kappa_0 M(r)}{r^2} \qquad (4,4.8)$$

for a point at the surface of a sphere containing the mass $M(r)$, the differential equation for R follows:

$$\frac{d^2 R}{dt^2} = \frac{-\kappa_0 M}{R^2}, \qquad (4,4.9)$$

where $M = \frac{4}{3}\pi\rho R^3$ is a constant; it is the mass contained in a volume of unit radius $r_0 = 1$. The mass remains unchanged within any expanding closed surface.

The differential equation immediately yields the integral

$$\frac{1}{2}\dot{R}^2 = \frac{\kappa_0 M}{R} + h, \qquad (4,4.10)$$

where the constant of integration h may be negative, positive, or zero. Accordingly, we obtain various types of expansions which may be called "elliptic," "hyperbolic," and "parabolic," since the character of the solutions closely resembles solutions obtained in the theory of rectilinear motion of two gravitating bodies.

In the preceding general discussion of the problem it has been shown that the dynamical problem of an expanding or contracting universe always give rise to singularities when, for $R = 0$, ρ becomes infinite. The singularities can be removed by amending Poisson's equation for the gravitational potential by an additional term.

D. The Cosmological Constant

If ϕ is supplemented by a term with the new constant λ,

$$\phi = \frac{\kappa_0 M(r)}{r} + \frac{\lambda}{6} r^2, \qquad (4,4.11)$$

the differential equation for the scale factor $R(t)$, all other assumptions left as before, becomes

$$\frac{d^2R}{dt^2} = \frac{\kappa_0 M}{R^2} + \frac{\lambda}{3} R \qquad (4,4.12)$$

and yields the integral

$$\tfrac{1}{2}\dot{R}^2 = \frac{\kappa_0 M}{R} + \frac{\lambda}{6} R^2 + h . \qquad (4,4.13)$$

The singularity $\rho \to \infty$, which formerly occurred for $R = 0$, does not appear again, if λ is chosen > 0, $h < 0$, and M is appropriately small.

In this case the equation

$$\frac{\kappa_0 M}{R} + \frac{\lambda}{6} R^2 + h = 0 , \qquad (4,4.14)$$

corresponding to the case $\dot{R} = 0$, yields two solutions, for, say, $R = R_1$ and $R = R_2$, where $R_1 < R_2$. For $R > R_2$, real solutions exist, for which R, coming from infinity—corresponding thus to a contracting universe—approaches the limit $R = R_2$, where $\dot{R}$ becomes zero. The contraction comes to a standstill for a regular

finite value of ρ and changes into an expansion; at this turning point the repulsive force just balances the attraction due to the general gravitational field of the substratum.

According as λ and h are chosen $\gtrless 0$ and the appropriate values for M are introduced, a great variety of solutions results. But only the one class, just mentioned, corresponding to $\lambda > 0$, $h < 0$, and M—that means also ρ—small, yields solutions which are free from singularities and hence do not necessitate the assumption that the universe has to pass through a critical phase. But it must be emphasized that such regular solutions are obtainable only by introducing the *ad hoc* hypothesis of a new and otherwise not traceable repulsive force in the universe.

5. The Relativistic Treatment of the Cosmological Problem

5,1. General Introduction

The relativistic approach to the cosmological problem proceeds apparently on such absolutely different lines that to arrive at the same final equations which describe the expansion may appear rather surprising. It is, however, no more surprising than the fact that the field equations of the general theory of relativity, which are framed in a completely different set of conceptions, lead in the first approximation to the same laws of motion as Newton's theory.

The fundamentally new feature in the relativistic approach is the initial establishment of a correspondence between the phenomenon of gravitation in space and the geometry of physical space. The universal gravitational field is no longer placed in an empty space endowed from some mysterious source with the special laws of Euclidean geometry. Metric field and gravitational field become different expressions of one and the same fact, namely that in a space filled with matter and energy individual bodies are no longer freely movable, owing to the phenomenon of gravitation. In the theory of relativity they move according to purely geometric principles through a space of continuously changing metrical properties;

in the old interpretation, gravitational forces, acting upon them in a space of presupposed metrical properties, moved them. In the classical theory there was thus a duality of gravitational field and geometrical properties of space, without any possibility of linking them explicitly together. For the geometry of space was not related to any physical phenomena, although the axioms of Euclidean geometry are actually based on a physical assumption, namely that all bodies are freely movable without change of size and shape. Now both fields are merged into one field.

The metric field, determined by the distribution of all gravitating matter in the universe, will be generally of a non-Euclidean character, and it is just this deviation from Euclidean geometry which we interpret as gravitation.

Since the gravitational field changes continuously when we proceed in space from one place to another, it must be characterizable by the differential expression of the line element which, according to Riemann's foundation of geometry, defines the metrics of space. Thus the problem of gravitation—in our case specifically the cosmological problem—is reduced to the question: What is the expression for the line element at an arbitrary point of an expanding universe filled in its whole extent with matter, when gravitation is geometrized along the lines followed by the general theory of relativity? In particular, since a closed finite space is characterized by a special expression for the line element, do we get a line element which indicates that our universe is finite?

We see that the geometrization of gravitation throws light immediately upon this fundamental question of cosmology. For this reason the relativistic treatment of the cosmological problem proves much more fruitful, even though, as we shall see, it does not yet give us a conclusive answer to this fundamental question.

Before entering into the specific cosmological problems, we shall investigate briefly the empirical background which justifies such a geometrization of gravitation.

That we are accustomed to use rectangular Cartesian co-

ordinates in which to formulate the laws of motion is merely a matter of convenience. Instead of fixing the position of a mass point in space by its three coordinates x, y, z, with reference to three straight, orthogonal axes, we can equally well use more general Gaussian co-ordinates x_i, $i = 1, 2, 3$. Expressed in such general coordinates, the distance, ds, between two infinitely near points takes the form:

$$d s^2 = \sum_1^3 g_{\mu\nu} d x^\mu d x^\nu , \qquad (1a)$$

where the coefficients $g_{\mu\nu}$ are functions of the coordinates x_i.

In special cases this expression may take the simple form

$$d s^2 = dx_1^2 + dx_2^2 + dx_3^2 . \qquad (1b)$$

If, in particular, Euclidean geometry is assumed to prevail in space, i.e., if the law of Pythagoras holds good for finite dimensions, then the finite distance s of any point of space from the observer, assumed to be at $x_1 = x_2 = x_3 = 0$, is given by

$$s^2 = x_1^2 + x_2^2 + x_3^2 . \qquad (1c)$$

To have based the laws of motion on the more general but also much more complicated expression (1a) of the line element would have meant a useless and unproductive complication of all formulae of physics unless the deviations from the Euclidean form (1b) found a natural physical interpretation. That could not be achieved as long as the metrical properties of space were considered to be of purely mathematical character. However, by superimposing a gravitational field upon a space of presupposed Euclidean geometry, as Newton's theory had to do, certain features were impressed upon the theory of motions which, from the outset, were felt to be extremely unsatisfactory in principle. For instance, the fact that all observations refer truly to bodies moving *relatively* to each other could not have been incorporated into the theory, except in the special case of uniform rectilinear motions. Furthermore, empty space had to be made responsible for the appearance of forces like the centrifugal forces, although they are indistinguishable from gravitational

forces on account of the apparently absolute equality of inert and heavy mass for every body.

It is the great discovery of Einstein's general theory of relativity that such fundamental shortcomings of the classical theory of motion can be eliminated by a geometrization of the gravitational field, i.e., by reducing the physical phenomenon of gravitation to the metrical properties of space.

Let us, for instance, consider an infinitely small volume of space near the earth's surface. Owing to the equality of inert and heavy mass, every falling body experiences the same acceleration in the gravitational field of the earth. In this small volume the effect of gravitation can be "transformed away" by a suitable transformation of the coordinates which cancels the acceleration; thus in this restricted area of space Euclidean geometry would be restored for an observer falling with all other bodies.

For a finite volume of space no such transformation is generally available. Owing to the change of the gravitational acceleration with increasing distance from the earth and owing to the gravitation of sun, moon, and planets, distant stars, etc., the gravitational acceleration cannot be simply transformed away. In geometrical terms this means that for an infinitely small volume of space the reduction of the general expression of the line element (1a) to the simpler Euclidean form (1b) is always possible—for finite dimensions, however, only if we presuppose the validity of Euclidean geometry in space.

Thus the expression for the line element,

$$d s^2 = \Sigma g_{\mu\nu} d x^\mu d x^\nu ,$$

can be used as an indicator of the existence of a gravitational field; the gravitational and metrical fields of space are then merged into one. The relativity of all motions may be postulated; the equality of inert and heavy mass, so far only empirically ascertained, becomes incorporated in the definition of mass; the unnatural duality in the definition of mass is avoided. Centrifugal phenomena, practically indistinguishable from

gravitational phenomena, become special manifestations of gravitation.

These very general remarks on the principles of general relativity need some essential amendments. First, all considerations have to be based on the four-dimensional line element

$$d s^2 = \sum_1^4 g_{\mu\nu} d x^\mu d x^\nu, \qquad (5,1.1)$$

including a fourth parameter, x^4, which may have the character of a pure time variable.

Second, the metric field of space is determined not only by the distribution of matter in space but also by the distribution and density of energy in its various forms, such as radiation, momentum, etc. Already the special theory of relativity has taught that energy is endowed with inertia, and the identity of the inert and heavy mass of every body which now becomes a cornerstone of the general theory of relativity thus attributes gravitation to all forms of energy.

In order to establish *de facto* the relationship between geometry and gravitation, the line element ds in its general form defined by ten functions $g_{\mu\nu}(\mu, \nu = 1, 2, 3, 4)$, $g_{\mu\nu} = g_{\nu\mu}$, has to be connected with the ten components of the mass-energy tensor which describes the distribution of all quantities on which the gravitational field depends. The gravitational field is no longer described by one function of scalar nature, the potential ϕ. It is described now by a tensor with ten components; the ten functions $g_{\mu\nu}$ replace the one potential ϕ. This makes the relativistic treatment of all dynamical problems, to which our cosmological problem also belongs, extremely intricate.

The cosmological problem approached within this new frame of ideas asks: What is the expression for the line element in a universe which is uniformly filled with matter while expanding symmetrically in all directions? Can we draw from the form of the line element general conclusions with regard to the structure of the world, in particular with regard to the alternative: finite or infinite world?

To obtain an answer to these questions, we must start from

the most general expression for the line element, i.e. (5,1.1), and apply, step by step, the various special assumptions which we assume to hold good in the universe.

First, since all gravitating matter is supposed to be uniformly distributed over the available space and since, moreover, the appearance of the universe is to be the same for every observer, the problem will be solved when the expression for ds is known for one and only one observer in an arbitrary position x^1, x^2, x^3, x^4, of the universe. Second, we shall assume that every observer measures his proper time t with a clock at rest in his position; his own coordinates, in a system of reference fixed to his position as origin, at any arbitrary moment will be: $x^1 = x^2 = x^3 = 0$, $x^4 = t$. Consequently, without any sacrifice in generality, we may start from the more simple expression:

$$d s^2 = dt^2 + \sum_1^3 g_{\mu\nu} d x^\mu d x^\nu . \qquad (5,1.2)$$

The coefficients $g_{\mu\nu}$ in the last equation might still be functions of all four variables x^1, x^2, x^3, t. Third, the observed expansion is the same in all directions; it will consequently affect the scale but not the metrical properties of space in the course of time. Changes of the $g_{\mu\nu}$ in time must therefore be describable in terms of a changing scale factor $R(t)$ acting upon all three co-ordinates in the same sense. We may consequently split the scale factor off and put

and

$$g_{\mu\nu} = -R^2 (t) \, \gamma_{\mu\nu}$$

$$d s^2 = dt^2 - R^2 (t) \, d \sigma^2 ,$$

where

$$d \sigma^2 = \sum_1^3 \gamma_{\mu\nu} d x^\mu d x^\nu \qquad (5,1.3)$$

depends on the three space coordinates only.

So far we have made use only of the cosmological postulate, of special assumptions concerning the distribution of matter in space, and of the character of the general expansion; these assumptions reduce the expression for the line element to the most simple possible form, given above. The field equations of the

theory of relativity, i.e., the equations which actually link the metrical functions $g_{\mu\nu}$ to the gravitational field, have not yet been used. These field equations, which will be considered more closely in the supplement to this section, can be considerably simplified. Of the ten components of '--; mass-energy tensor which defines the gravitational (inertial) field, we can disregard all but one component, namely, the one which enters also into the classical theory. It is the density ρ of the cosmic matter in space. This is justified because the gravitational field arising from the radiation filling the interstellar space is negligibly faint compared with the field produced by solidified matter. The same is true of the field originating from the energy manifested in the irregular random motions of the cosmic matter, i.e., the spiral systems. Thus the only quantity which is made responsible for the observed (inertial) field is the density of matter which we find condensed to stars in the star-systems. With these restrictions, the field equations take a particularly simple form, from which very general and, for the cosmological problem, essential conclusions can be drawn. One equation assumes the simple form

$$3\frac{\ddot{R}}{R} = \tfrac{1}{2}x\rho \,,$$

where $\ddot{R}$ is written for d^2R/dt^2. This equation does not contain the metrical functions $\gamma_{\mu\nu}$ at all; and, since the left-hand side depends on the time t only, we may conclude immediately that the right-hand side, i.e., the density ρ, is also a function of the time t only.

We arrive thus, as in the classical case, at the result that the matter filling the universe must be uniformly distributed, the density being a function of t only. In the remaining field equations, which contain the metrical functions $\gamma_{\mu\nu}$ and their derivatives, the latter appear cloaked in a tensor $G_{\mu\nu}^{*}$ in such a way that the conclusion $G_{\mu\nu}^{*}/\gamma_{\mu\nu} = \text{const.}$ may be drawn immediately. This equation, however, is known as the equation to be satisfied in a space of constant curvature. Whether the curvature is positive and space consequently closed and finite, or negative,

or zero (in which latter cases space would be of infinite volume) remains still an open question. The answer would depend on a final solution of the field equations, which, however, is not to be expected at present. For, apart from the expansion term $\dot{R} = dR/dt$, the equations contain also terms containing $\ddot{R} = d^2R/dt^2$, i.e., terms depending on the acceleration (or deceleration) of the expansion, about which so far nothing is known.

But the relativistic treatment promises to give us a more direct insight into the one alternative so fundamental in the cosmological problem, namely, that of the finiteness or infinity of space. Apart from these general conclusions (see supplement), one integral can be derived from the field equations which, in appropriately chosen constants, takes the same form as the integral obtained for the scale factor R in the classical case; it reads:

$$\tfrac{1}{2}\dot{R}^2 = \frac{\kappa_0 M}{R} + \text{const.}$$

The only difference is that in the classical treatment the constant on the right-hand side appeared as, apart from numerical factors, an unspecified constant of integration, h, the energy constant. In the relativistic treatment the most interesting quantity is a factor ϵ, which can assume any of the three values

$$\epsilon = \begin{cases} +1 \\ 0 \\ -1 \end{cases},$$

which decide whether space is finite or infinite.

The fact that in both treatments we obtain the same equation for R is an interesting, but by no means surprising, result. The expansion of the universe is in both cases explained according to the rules which govern regular motions; in the classical case it is reduced to a known problem in celestial mechanics, in the relativistic treatment the field equations must lead in the first approximation to laws of motion similar to those of classical mechanics. Thus both treatments could be expected to yield similar results in the first approximation.

From this point on the discussion follows therefore, the same line as in the classical case. We have to distinguish again between solutions of the elliptic, parabolic, and hyperbolic types, the first class being of a periodic character. Also the same singularities appear when R, tending to zero, causes ρ to increase beyond all finite limits.

If we summarize the results obtained so far, we see that the relativistic treatment does not produce, as far as the representation of observable facts is concerned, results which extend beyond those obtainable also by classical methods. However, there can be no doubt that the relativistic approach promises to bring us much nearer to the roots of the whole problem, as soon as the empirical data are complete enough to make possible an exhaustive treatment of the field equations. For the equations lead immediately toward an answer to the fundamental question: Is space finite or infinite? This gain is due to the geometrization of gravitation, which automatically includes a geometrization of the cosmological problem, too.

The singularities which enter as a consequence of the dynamical character of the solution may be treated in the relativistic case in a manner very similar to the classical case. In the latter they were removed by adding to Poisson's equations for the gravitational potential a term with a new arbitrary constant λ, now called the "cosmological constant." Such a term means the introduction of a repulsive force which has to counterbalance the effect of gravitation.

Although the removal of the singularities proceeds in the relativistic treatment along similar lines, there are distinguishing features of a very essential character.

Both the amendment of Poisson's equation and that of the field equations in the relativistic theory were originally introduced not to remove singularities which appear in the *dynamical* problem of an expanding universe but to remove the difficulties which were encountered when the aim was to establish the existence of a static, infinite world. Einstein's first solution of the cosmological problem in 1917 still aimed at a static solution. The general red-shift of the spectral lines of distant

star-systems, i.e., the expansion of the universe, was not yet known. In both the classical and the relativistic case the static solution encounters difficulties which make it necessary to introduce additional terms into the equations connecting the potential ϕ and the metrical functions $g_{\mu\nu}$. Now a static world is no longer asked for. The amendment has to remove the singularities entering the dynamical solution. A λ-term, supplementing Poisson's equation of the Newtonian potential, added to the left-hand side of the relativistic field equations serves this purpose. The introduction of such a term is in both cases an *ad hoc* hypothesis. In the relativistic treatment, however, the new term can be incorporated in a more natural way.

Poisson's equation, which states that the divergence of the gravitational force through a closed surface is proportional to the density of the enclosed gravitating matter, loses, by the addition of the λ-term, the possibility of such an interpretation. The field equations of general relativity, on the other hand, in which the components of the matter-energy tensor are set proportional to a differential expression of the second order in the metrical functions $g_{\mu\nu}$, (μ, ν = 1, 2, 3, 4), and their derivatives, permit, without restricting their applicability, the addition of a term $\lambda g_{\mu\nu}$; this drops out when the divergence is calculated. Thus a cosmological constant λ may be introduced into the relativistic treatment without any fundamental influence upon the theory. In addition, the term has a purely metrical meaning, apart from representing a repulsive force counteracting in large dimensions the action of the universal gravitational field. From the metrical point of view the λ-term enables us to calibrate all measurements of lengths in a finite universe, when both λ and ε are chosen as >0. The constant radius of curvature in this model of the universe defines the scale to which the length of the meter as the unit of length may be referred. And, since the field equations in their most general form definitely require the addition of a λ-term, its introduction into the theory of relativity is claimed by some—for instance, by Eddington—to be an essential and indispensable feature of the theory. On the other hand, it cannot be denied that, owing to the fact that no further evidence supports the introduction of such a term, the

beauty of the relativistic theory is to a certain extent disturbed by such a term. Without the urge of the cosmological considerations, the relativistic theory could rightly boast of being able to account for all observed facts without introducing any constants apart from the known fundamental constants of nature, i.e., the gravitational constant κ and the value of the light velocity c. The λ-term upsets this claim. This and the fact that the inclusion of the λ-term actually does not yet give a final solution of the cosmological problem have induced Einstein and others to drop this term completely. They thus sacrifice, perhaps, very attractive solutions of the cosmological problem, but they free the theory from not sufficiently well-founded assumptions.

When, however, the λ-term is accepted, the integral derived from the field equations (see below) takes a new form; in appropriate units it reads,

$$\tfrac{1}{2}\dot{R}^2 = \frac{\kappa_0 M}{R} + \frac{\lambda}{6} R^2 c^2 - \tfrac{1}{2} \epsilon c^2, \qquad (5,1.4)$$

where c is the velocity of light and $\epsilon = \begin{cases} +1 \\ 0 \\ -1 \end{cases}$ is the above-mentioned factor of which decides whether the expression for the line element at any position in the universe corresponds to that of a closed finite space of positive curvature ($\epsilon = +1$), or of a Euclidean flat space ($\epsilon = 0$), or of a hyperbolic space ($\epsilon = -1$). The new form of the integral, which was also obtained in the classical case, after amending Poisson's equations by a λ-term, offers solutions which are free from the disturbing singularities encountered without the λ-term. When λ is chosen > 0, $\epsilon = +1$, and when also the value of M is chosen appropriately, solutions result which no longer lead to a catastrophic collapse of the universe for $R = 0$. In addition, solutions are obtained, when $\epsilon = +1$, which yield a closed and finite universe.

5,2. Supplement

In the general theory of relativity the scalar potential ϕ of the gravitational field is replaced by the ten functions $g_{\mu\nu}$ in the expression of the line element

$$ds^2 = \sum_{1}^{4} g_{\mu\nu} dx^\mu dx^\nu .$$

The density ρ of the matter filling space has to be replaced by the ten components of the energy-momentum tensor

$$T_{\mu\nu} = \rho \sum_{\alpha\beta} g_{\mu\alpha} g_{\nu\beta} \frac{dx^\alpha}{ds} \frac{dx^\beta}{ds} , \qquad (5,2.1)$$

for not only matter but all forms of energy contribute to establishing the universal inertial field.

Poisson's equations for the potential ϕ are replaced by the field equations of the theory of relativity, which postulate that

$$G^{\mu\nu} - \tfrac{1}{2} g^{\mu\nu} G = - \kappa T^{\mu\nu} .$$

On the left-hand side the metric field is represented by the components of a tensor, containing the $g_{\mu\nu}$ and their first and second derivatives,

$$G^{\mu\nu} - \tfrac{1}{2} g^{\mu\nu} G ,$$

which satisfy the equation

$$\text{div} \, (G^{\mu\nu} - \tfrac{1}{2} g^{\mu\nu} G) = 0 .$$

The $G^{\mu\nu}$ are the components of a tensor called the Riemann-Christoffel tensor; G is the invariant derived from it: κ is a constant connected with the constant of gravitation κ_0 by $\kappa = (8\pi/c^2) \kappa_0$; c is the velocity of light. The field equations may also be written in the form:

$$G_{\mu\nu} = - \kappa \, [T_{\mu\nu} - \tfrac{1}{2} g_{\mu\nu} T] .$$

These equations lead in the first approximation to Newton's laws of motion in a gravitational field. Terms of higher order predict the correct amount of the advance of the perihelion of Mercury which could not be explained in the classical theory.

Into these field equations we have to introduce the empirical facts furnished by astronomical observations and have to decide which values must be given to the components of the energy-momentum tensor in a world over which the matter is spread at every moment with a constant mean density ρ. It is beyond doubt that in the first approximation the gravitational

field arising from the radiation filling stellar and intergalactic space may be completely disregarded; also the field arising from the momentum of the streaming matter need not be considered. Thus, apart from the component $T^{44} = \rho$, none of the ten components of the energy-momentum tensor

$$T^{\mu\nu} = \rho u^{\mu} u^{\nu} \qquad \mu, \nu = 1, 2, 3 ,$$

where

$$u^i = \frac{d x^i}{d s} ,$$

needs to be taken into account. Only $T^{44} = \rho$, the density of the matter, remains, as in the classical case.

But the density ρ may still be a function of the time $x_4 = t$ and of the spatial coordinates; i.e., $\rho = \rho(x^i, t)$.

With regard to the line element, (5,1.1), considerable simplifications are also possible, without reducing the generality of the following considerations. We may treat the world as a "3 + 1 dimensional" manifold, splitting off the "time-term," and write

$$d s^2 = d \tau^2 + \sum_1^3 g_{\mu\nu} d x^{\mu} d x^{\nu} , \qquad (5,2.2)$$

where in common units

$$d \tau = c d t .$$

The $g_{\mu\nu}$ may still be functions of all four coordinates: x_1, x_2, x_3, t. This line element allows a further reduction to a more simple form when we consider that the observed expansion is the same in all directions. Every spatial direction is consequently absolutely equivalent to any other; in other words, space is *isotropic*. This reduces the $g_{\mu\nu}$ to the special form:

$$g_{\mu\nu} = - R^2 (\tau) \gamma_{\mu\nu} ,$$

and the line element takes the final form:

$$d s^2 = d \tau^2 - R^2 (\tau) - d \sigma^2 , \qquad (5,2.3)$$

where

$$d \sigma^2 = \sum_1^3 \gamma_{\mu\nu} d x^{\mu} d x^{\nu} ,$$

and R, representing a universal scale factor, is still an unknown

function of the time τ. Since every observer is expected to experience a completely identical picture of the universe, the knowledge of ds at only one arbitrary point of the universe is sufficient for a complete solution of the cosmological problem.

The differential equation for R, which describes the expansion, is obtained by introducing the special values of $T^{\mu\nu}$ and $g_{\mu\nu}$, just derived, into the field equations, after having calculated the values of the $G^{\mu\nu}$. Two differential equations of second order in R result; they read

$$G^*_{\mu\nu} - 2\dot{R}^2\gamma_{\mu\nu} - R\ddot{R}\gamma_{\mu\nu} = -\tfrac{1}{2}\,\kappa R^2 \rho\,\gamma_{\mu\nu} \qquad (5,2.4)$$

and, for the term for which $\mu = \nu = 4$,

$$3\,\frac{\ddot{R}}{R} = -\tfrac{1}{2}\,\kappa\rho\,, \qquad (5,2.5)$$

where $\dot{R}$ and $\ddot{R}$ denote the first and second derivatives of R with regard to the time; $G^*_{\mu\nu}$ denotes a tensor resulting from contraction of the Riemann-Christoffel tensor with regard to the three spatial coordinates only.

From these equations two essential conclusions can immediately be drawn. Since R is a function of τ only, it follows from equation (5,2.5) that the right-hand side also must be a function of τ only; hence $\rho = \rho(\tau)$, the density of the matter, is at any fixed moment the same at every point of space. This is the same result as that which in the classical treatment in section 4,2 followed from Poisson's equations for the potential ϕ.

Moreover, if we divide equation (5,2.4) by $\gamma_{\mu\nu}$, we obtain

$$\frac{G^*_{\mu\nu}}{\gamma_{\mu\nu}} = 2\dot{R}^2 + R\ddot{R} - \tfrac{1}{2}\,\kappa R^2\rho\,. \qquad (5,2.6)$$

Here the left-hand side is a function of the space coordinates x^i ($i = 1, 2, 3$) only, the right-hand side of the time t only; from that we conclude

$$\frac{G^*_{\mu\nu}}{\gamma_{\mu\nu}} = \text{const.}$$

This equation, written in its general form,

$$G^*_{\mu\nu} + (n-1)C\,g_{\mu\nu} = 0 \qquad (5,2.7)$$

is the necessary condition for a constant curvature of space; C is a constant; n, the number of dimensions, is in our case equal to 3.

Therefore, under the special assumptions concerning the distribution of matter and the cosmological postulate, the field equations yield the preliminary result that the three-dimensional space must be one of constant curvature. We can write the last equation in the form

$$G_{\mu\nu}^* + 2\epsilon\, C'\, g_{\mu\nu} = 0 \ , \qquad (5,2.8)$$

C' representing a positive constant; then $\epsilon = +1$ corresponds to a spherical or elliptic space of finite content, $\epsilon = 0$ to infinite Euclidean space, and $\epsilon = -1$ to infinite hyperbolic space. No indication is so far given as to which of these three cases our universe actually belongs to.

The law of expansion, giving R as a function of τ, follows by the elimination of the quantities depending on the spatial coordinates from (5,2.4–8), that is, by eliminating G_{μ}^* and $\gamma_{\mu\nu}$. Introducing into equation (5,2.4) the value of $G_{\mu\nu}^*/\gamma_{\mu\nu}$ from equation (5,2.8) and replacing from equation (5,2.5) the term $R\ddot{R}$ by $-\tfrac{1}{3}kR^2\rho$, we obtain the equation

$$\dot{R}^2 = \tfrac{1}{3}\,k\,\rho R^2 - \epsilon - C' \qquad (5,2.9)$$

Differentiating this equation and replacing the terms containing $\ddot{R}$ again from equation (5,2.2) produces

$$\frac{\dot{\rho}}{\rho} + 3\frac{\dot{R}}{R} = 0 \ .$$

This last equation can be written

$$d \log (\rho R^3) = 0$$

and yields the integral

$$\tfrac{4}{3}\,\pi\,\rho R^3 = M = \text{const.} \qquad (5,2.10)$$

The arbitrary numerical factor $4\pi/3$ has been attached to make the constant M equal to the corresponding constant M which resulted in the classical case from the corresponding integration of the differential equation of the scale factor R (see p. 22).

The integral expresses the conservation of matter and energy. If we introduce this value into equation (5,2.4), we obtain

$$\dot{R}^2 = \frac{\kappa M}{4\pi R} - \epsilon C'.$$

To reduce this equation to the common units used in the differential equation in the classical case (sec. 4,2), $d\tau$ has to be replaced by cdt and κ by $(8\pi/c^2)\kappa_0$. The resulting equation,

$$\tfrac{1}{2}\dot{R}^2 = \frac{\kappa_0 M}{R} - \tfrac{1}{2}\epsilon c^2 C', \qquad (5,2.11)$$

is the same as the equations obtained in the discussion based on Newton's law of gravitation. The further discussion proceeds, therefore, on parallel lines to those of art. 5,1.

5,3. The Cosmological Constant

If we definitely wish to free the model of our world from any possible catastrophe, the singularity which appears in the solutions of the last question can be removed in a similar way by a λ-term. The tensor $G^{\mu\nu} - \tfrac{1}{2}g^{\mu\nu}G$, which enters into the field equations, may be complemented by an additional term of the form $\lambda g^{\mu\nu}$.

λ is the "cosmological constant" originally introduced by Einstein into the problem and later abandoned. The field equations now read

$$G^{\mu\nu} - \tfrac{1}{2}g^{\mu\nu}G + \lambda g^{\mu\nu} = -\kappa T^{\mu\nu}, \qquad (5,3.1)$$

and equations (5,2.4) and (5,2.5) have to be replaced by

$$G^*_{\mu\nu} - 2\dot{R}^2\gamma_{\mu\nu} - R\ddot{R}\gamma_{\mu\nu} + \lambda R^2\gamma_{\mu\nu} = -\tfrac{1}{2}\kappa\rho R^2\gamma_{\mu\nu}, \qquad (5,3.2)$$

$$3\frac{\ddot{R}}{R} - \lambda = -\tfrac{1}{2}\kappa\rho. \qquad (5,3.3)$$

The new differential equation for R reads:

$$\dot{R}^2 = \tfrac{1}{3}\kappa\rho R^2 + \frac{\lambda}{3}R^2 - \epsilon C, \qquad (5,3.4)$$

which, after again introducing cdt for $d\tau$ and the gravitational constant, κ_0, and dropping superfluous constants, gives us

$$\tfrac{1}{2}\dot{R}^2 = \frac{\kappa_0 M}{R} + \frac{\lambda}{6}R^2 c^2 - \tfrac{1}{2}\epsilon c^2, \text{ i.e. (5,1.4)}.$$

While formerly $\frac{4}{3}\pi\rho R^3 = M = \text{const.}$ inevitably involved a singularity of $\rho \to \infty$ for $R = 0$, the solutions of the new equation no longer necessarily have a singularity of this kind, either in the past or in the future. The amended integral offers nine separate classes of solutions corresponding to the possible combinations of $\lambda \gtrless 0$ and $\epsilon \gtrless 0$; among them are, for $\lambda > 0$, $\epsilon = +1$, and M appropriately chosen, solutions free of the disturbing singularities.

5,4. General Remarks concerning Singularities in the Cosmological Problem

As far as the representation of observable facts is concerned, neither the classical nor the relativistic treatment can claim superiority over the other for the cosmological problem. Both lead to the same main results: that a static world filled with gravitating matter is not possible and that an isotropic expansion is compatible with the cosmological postulate and describable in terms of their laws of motion; both treatments lead in the first approximation to the same equation describing the expansion.

The only disturbing factor is the appearance of singularities which make it impossible to apply the solution to the whole lifetime of the universe. In the classical case, such singularities can be avoided only by a departure from Newton's law of gravitation—a very serious step; in the relativistic case the theory is flexible enough to allow the necessary amendments. However, they introduce into the laws of nature a new universal constant, the cosmological constant λ, for which there appears otherwise no necessity.

We must ask therefore, since the singularities arise when we get excessively large values of the density ρ, whether our fear of excessive densities, the opposite to the *horror vacui*, is justified and not perhaps the last reminder of a subconscious yearning for a harmonious universe. If all observable facts in our present universe were intelligible, we would not have any reason to suspect that the universe has gone through conditions essentially different in the past from those we observe at

present, conditions which we would call "singular." But the universe definitely reveals features which we are so far unable to explain within the frame of the actually accepted laws of nature.

The matter in space from which the value of the density ρ in all preceding considerations has been derived appears condensed in radiating stars and in clouds of a widely dispersed dark matter. In the stars the matter is highly condensed at a high temperature; in the clouds both temperature and density are very low. But both forms of matter contain atoms of large atomic weight, i.e., atoms heavier than hydrogen and helium.

According to atomic physics, it seems securely established that the atoms of all chemical elements are built up from protons, neutrons, and electrons and that in the building-up of the various elements large amounts of atomic energy are freed. The latter fact offers a solution to the problem of the source of energy of stellar radiation. In particular, the transformation of hydrogen into helium proves to be a source which, at the temperature prevailing in the central cores of many stars, yields sufficient energy to supply their radiation. Hydrogen becomes the prevalent element in the early stages of stellar life.

In accordance with this fact, the theory of the internal structure of stars was forced to assume a high percentage of hydrogen in stellar matter to remove discrepancies which otherwise would have arisen between astrophysical observations and atomic physics. However, the creation of helium from hydrogen goes on only in the presence of sufficient amounts of nitrogen and carbon. No difficulties in principle would arise if, apart from the building-up of helium, the building-up of heavier atoms like nitrogen and carbon also appeared possible in the stars. This is, however, not the case. Temperatures higher than $2 \cdot 10^7$ degrees in the center of a star of solar mass are not to be expected, and this value is far too low to account for the building-up of atoms heavier than helium. Thus the origin of heavy atoms as they are found in the earth's crust, the sun, and all stars in roughly the same relative abundance remains inexplicable in the present state in which we find the stellar matter.

Temperatures of the order of 10^{10} degrees would be needed. We have reason, therefore, to assume that the universe passed through a stage when the heavy elements were created. Temperatures and densities must have been much higher than the values which we experience in the present universe.

Also the fact that all estimates of the age of the universe (see the paragraph on p. 56) point toward a surprisingly short time scale of the order of only 10^9–10^{10} years leads to the conjecture that the present universe is in a temporary state which may be separated from a preceding state by a critical phase of transition. Our efforts to remove singularities from the solution of the cosmological problem may therefore prove to be vain and unnecessary attempts.

5,5. The Importance of the Conception of a Finite World

The one fundamentally new feature introduced by the relativistic theory is the existence of solutions of the field equations yielding a closed finite world. It is a problem of the greatest importance for future investigations to make sure whether the distribution of matter in space indicates a line element corresponding to such a spherical or elliptic space. In the next section we shall discuss this question with regard to the results derivable from present observations.

The first solution of the cosmological problem in the general theory of relativity by Einstein in 1917, which still aimed at a static world—the general expansion of the universe was then still unknown—already offered such a solution. It brought for the first time the concept of a finite world within the reach of exact scientific research. Although this special solution is no longer of practical interest, because it does not account for the expansion, besides corresponding to an unstable world, it marks a milestone in the advance of the cosmological problem. The inclusion of the general expansion does not deprive the problem of such solutions; they correspond now to expanding or contracting closed spaces.

If we knew that our universe was finite and thus contained only a finite amount of gravitating matter, the delicate problem

of infinity would be definitely removed from the realm of physical science. In addition, such a model of the world would open the way to a deeper understanding of the intrinsic properties of mass, gravitation, and inertia.

Mach had already realized that the property of inertia, i.e., of inert mass, manifested by every body and strictly proportional to its gravitating mass, should be reducible to the universal gravitational field produced by all matter filling the world. One cannot single out two bodies and attribute to them certain physical properties, as, for instance, inert mass, without keeping in mind that these properties must be brought into being by the very existence of all the other masses in the world. Mach says, without explicit reference to the cosmological problem, in chapter ii of his *Mechanik*: ". . . one has to realize that even in the most simple case, when apparently only the action between two bodies is considered, it is impossible to disregard the rest of the universe. Nature does not start from elements as we are forced to begin with. . . ." So we have to expect from a fully satisfactory solution of the cosmological problem a final answer also as to the question of the origin of inertia. Obviously, however, only in a finite world can we hope to relate the finite inertia of every body to the field induced by the finite total amount of gravitating matter in the universe.

Indeed, from the preceding equations (5,3.2), (5,3.3), and (5,3.4), in the special case: $\dot{R} = \ddot{R} = 0$ and $\epsilon = +1$, we obtain $2/R^2 = \kappa\rho$, and $\lambda = 1/R^2$. In this case the finite radius of curvature of the world is determined by the mean density of the matter which produces the universal field of inertia. No boundary conditions for infinity are needed because the finite density of matter in our model produces a finite world and abolishes the necessity of defining boundary conditions at infinity for the gravitational "potentials" $g_{\mu\nu}$.

The gravitating matter of the universe *produces*, so to speak, the space of the universe, and all manifestations of matter, like inertia, are defined only with regard to the gravitational field set up in this space.

In the static solution to which Einstein was led, the mean

density ρ alone defines the value of the radius of curvature. For nonstatic solutions—and obviously they alone apply to the actual world—$\dot{R}$ and $\ddot{R}$ are no longer zero and must be known. This complication explains why the observational test of the relativistic theory of an expanding world is still incomplete.

5,6. The Observational Test of the Relativistic Theory

The necessity of admitting a new parameter λ, without being able to fix its sign and without, in addition, knowing the sign of ϵ in the expression for the line element, increases the multitude of possible solutions to such an extent that at present we are still unable to select out of this multitude the solution referring to the actual universe. Could we disregard the λ-term, i.e., put $\lambda = 0$, then we could immediately conclude from equation (5,3.3) that

$$\dot{R}^2 = \tfrac{1}{3} kR^2\rho - \epsilon$$

which gives

$$\tfrac{1}{3} k\rho - \left(\frac{\dot{R}}{R}\right)^2 = \frac{\epsilon}{R^2}. \tag{5,6.1}$$

The geometry of a finite and closed space, $\epsilon = +1$, obviously entails

$$\rho > \frac{3}{k}\left(\frac{\dot{R}}{R}\right)^2,$$

while

$$\rho < \frac{3}{k}\left(\frac{\dot{R}}{R}\right)^2$$

corresponds to a metric of the hyperbolic type. The knowledge of the mean density ρ and of the expansion $\dot{R}$ would suffice to decide whether the world may be supposed to be finite or not. The observed rate of expansion yields

$$\frac{\dot{R}}{R} = \frac{1}{c}\frac{525 \text{ km sec}^{-1}}{10^6 \text{ parsecs}} = 5.68 \cdot 10^{-28} \text{ cm}^{-1}.$$

If we introduce this value into the preceding inequalities, we find

$$\rho \gtrless 5.2 \cdot 10^{-28} \text{ gm cm}^{-3}.$$

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The observational data, on the other hand, yield a value for ρ, when we spread all matter, condensed in spiral systems, evenly over space, only of the order of 10^{-31} gm cm^{-3}. For the survey of spiral systems gives about 10^8 systems in a sphere of about $3 \cdot 10^8$ parsecs diameter; each system contains about 10^9 stars of solar mass; hence each system has a total mass of about $2 \cdot 10^{42}$ gm. This gives a mean density,

$$\rho = \frac{2 \cdot 10^{50}}{4.4 \cdot 10^{80}} \cong 5 \cdot 10^{-31} \text{ gm cm}^{-3}.$$

This value would indicate an infinite world with a hyperbolic metric. But, in fact, the observational data are, as yet, far too uncertain to permit any such far-reaching conclusions.

Moreover, when the statistics of nebulae reach distances from which the light needs more than, say, 10^8 years to travel to the observer, the expansion of the universe may have undergone, during that time, an acceleration or deceleration which might not be negligible; thus terms containing $\ddot{R}/R$ may have to be considered. The formulae which represent the observed facts become very complicated; they have to be developed into series, and a careful estimate of the contribution of each term is needed. Various discussions on these lines have been carried out, using all available data. They show that the observational data are not yet sufficiently reliable to justify such an ambitious proceeding. The results of different discussions, based on practically the same material, are inconsistent with one another. An undetected, systematic error in the photometric scale of only 0.1 magnitude, for instance, for magnitudes larger than 19, could falsify the resulting photometric distances sufficiently to annul an otherwise clearly indicated positive curvature of space. Thus no final conclusion concerning the sign of the curvature of space or the sign of the cosmological constant can yet be drawn.

The main conclusion which so far may be drawn from a comparison of theory and observations is as follows:

The relativistic treatment of the cosmological problem promises to give in the future a definite answer to the one question

which appears to be the highest prize of all efforts, namely, the question: Is the universe closed and finite? The possibility that the universe may be closed and finite is not excluded by the observational facts but neither, so far, is it supported by them. Solutions of the cosmological problem which lead to a finite world demand values for the mean density ρ of matter about a thousand times higher than observations indicate. Moreover, the value of the constant radius of curvature of space resulting from these solutions is not much larger than the greatest distances which have, so far, been actually reached, i.e., distances of the order of 10^8–10^9 parsecs. If the universe were really so small, the present survey would already cover about one-fourth of its whole extent. This appears extremely unlikely. We would, moreover, be forced to assume that the matter which we observe concentrated in the star-systems is only a small fraction of the matter that is responsible for the universal gravitational field and thus for the curvature of space. A mean density ρ of the order of 10^{-31} gm cm^{-3}, in accordance with the observational facts, does not justify the conclusion that the metric of space is of a spherical character. If the λ-term is completely dropped, we obtain a solution proposed by Einstein and De Sitter, corresponding to an expanding world with Euclidean geometry.

The distribution of matter in our neighborhood up to 10^8 parsecs seems to agree perfectly with a uniform distribution of matter in space only if we do not interpret the red-shift as a Doppler effect. The corrections which, according to section 3,3, must be applied if the red-shift is interpreted as a Doppler effect change the apparent distribution of matter so significantly that the assumption of a uniform distribution of matter in space no longer seems to hold good. An agreement with the assumption of uniformly distributed matter can be restored by introducing a positive curvature of space which just cancels the "disturbing" effect of the red-shift correction, if the red-shift is interpreted as a Doppler effect. The introduction of such a positive curvature of space offers a solution which yields a closed finite world. But this picture of the world seems hardly acceptable on account of its small size and the high density of

matter, a density about a thousand times higher than that which observations seem to allow. Therefore, if we accept the present observed value of ρ, we seem to be forced to sacrifice the most precious prize, that of the finiteness of the universe. But it must be emphasized once more: the observational data are still quite incapable to carry the responsibility for such a heavy burden of theoretical conclusions.

All considerations so far have been of a preliminary character, simplifying to the utmost all assumptions concerning the distribution of matter in space and concerning its motion. It may turn out that, just as our knowledge was insufficient when we tried to solve the cosmological problem with a scanty knowledge of the distribution of stars in the galaxy, so now our present knowledge of the distribution of galaxies in the universe is not yet extensive enough to be adequate.

This, however, must not obscure the fact that the relativistic treatment of the cosmological problem is very much superior to the classical treatment. It opens up a fascinating insight into the complex relations which exist between the geometry of physical space and the forces of the material world. On this new basis of conceptions we may hope to obtain in the future a unified picture of the universe in which the mass, size, and geometry of the universe follow from a firmly established theory of matter and energy.

6. Concluding Remarks on the Time Scale of the Universe

The discovery of the expansion of the universe has given a very unexpected impetus to the problem of the time scale of the universe.

If we date the age of the universe from the beginning of its present expansion, it would apparently only slightly surpass the lower limit of $1.5 \cdot 10^9$ years, which is the age of the earth's crust. This assumes that the expansion proceeded steadily from zero to infinity. The result would not be substantially changed if our world belonged to the class of oscillating universes. A possible exception is offered by a model world ex-

panding asymptotically from the nonstable static solution of Einstein. No hierarchic structure in the ages of the various systems, earth—star—star-system—universe, seems so far to exist. This evidently means that in the not so very distant past the present state of our universe started from a very special event which we may have to call a "singularity." This event forms the limiting frame of our picture of the world. Other facts known and already mentioned make it equally probable that our present picture of the universe does not cover its whole lifetime.

The final solution of the cosmological problem depends on further extension of our empirical knowledge of the structure of the universe. Observational astronomy will have to furnish this knowledge. It is in itself, however, an advance of fundamental importance that the whole cosmological problem, including the question "Is space finite?" has been brought into the sphere of the exact reasoning of natural science.

Notes

1. The brightness of a star is given in magnitude classes; the intensity of light received from a star of magnitude m is 2.5 times that received from a star of magnitude $(m + 1)$. The zero point of measurement is arbitrarily fixed. The absolute magnitude of a star is the magnitude it would have if placed at the standard distance of 10 parsecs. 1 parsec $= 3.08 \cdot 10^{13}$ km.

2. When space is considered uniformly filled with gravitating matter, the mass contained in a small element dV of space will be equal to ρdV, where ρ measures the density of the distribution of matter; ρ may be in the most general case a function of the three space coordinates and time. The gravitational force exerted by any such element of mass upon an arbitrarily chosen point P at a distance r from the element is, according to Newton's law, inversely proportional to the square of r, having in each direction of coordinates a component X, Y, Z. The function

$$\phi = \int_V \rho \, \frac{dV}{r}$$

is called the "potential of all attracting particles." The components X, Y, Z, in terms of ϕ take the simple form:

$$X = \frac{\partial \phi}{\partial x}; \qquad Y = \frac{\partial \phi}{\partial y}; \qquad Z = \frac{\partial \phi}{\partial z}.$$

For a point P located deep inside the gravitating matter, the gravitational pull in any direction may be equal to that in the opposite direction. In this case a body placed at P will not suffer any acceleration, but it will be subject to a stress arising from the forces pulling it in all directions.

3. See, for details, sec. 3,2.

4. See E. Hubble, *The Observational Approach to Cosmology* (1937), Fig. 3, p. 57.

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Foundations of Biology

Felix Mainx

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Contents:

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I. Introduction[1]

The word 'biology' is here to be understood in the sense of the fundamental science of living natural objects and thus as denoting all the disciplines of zoölogy, botany, physical anthropology, and the parts of neighboring sciences which are relevant to this field. Applied biology in its various branches, medicine among others, will be excluded. Human psychology, which, on account of the peculiarity of its special methods, requires a separate treatment, is also excluded. The notion of biology is not to be set up in opposition to the sciences of zoölogy, botany, etc., nor are these sciences to be subordinated to it, as sometimes happens (see Chap. III, B). Understood in this sense, biology is a branch of natural science, and it will be shown in the following pages that in its structure, in the kind of statements it contains, and in its mode of work, it represents an empirical science like the other sciences of nature.

The delimitation of biology according to its subject matter is a purely practical question. The concept of life was originally derived from the subjective awareness of human beings and then applied later to a greater and greater range of natural objects, when it was thought that these had properties in common with human behavior. In the present state of knowledge there is in practice no difficulty in distinguishing living objects from dead ones. Where there may be some doubt, as in the case of the viruses, it is purely a matter of convention whether, for scientific purposes, we classify such things under biology or not, just as drawing a boundary between zoölogy and botany is purely a matter of convention. Some authors attach great importance to

establishing the boundary between the living and the dead by definition and, from a metaphysical need, wish to treat this question as an ontological one (see Chap. III, B, 4). Even if we find that drawing a sharp boundary is not empirically possible, the independence of biology as a science will not disappear. Its independence rests on the observed peculiarity of its object and on the development of its own methods of research and points of view, which are required by that object. The division of biology into its various branches is also purely practical and for that reason depends, at a given epoch, on the prevailing direction of interest among the various possible points of view. Also for that reason, it is not meaningful to ascribe a fundamental importance to a "system" of biology. The vague separation into zoölogy and botany, according to the kind of object studied, is traditional and therefore still largely provides the basis for the organization of teaching and research. The subdivision into morphology, physiology, genetics, ecology, etc., according to point of view or direction of approach, cuts across both realms of living things. The branches of biophysics, biochemistry, paleontology, biogeography, etc., form bridges to the related sciences, so that a sharp delimitation of biology from other sciences from the point of view of method is not possible, either in the practical work of research or in scientific organization. Efforts to achieve a synthesis, particularly on the border lines of biology, are very active and successful at the present day.

II. Ways of Work in Biology

A. General Foundations

Corresponding to the character of biology as an empirical science, its statements have the general features of empirical statements. In the face of the great variety with which we are confronted by the world of living things and by the problems which this variety presents, the part played by pure description among biological statements is very great. The elementary con-

stituent of a descriptive statement is the report of an act of observation—for example, the report of a color, of the measurement of a magnitude, of counting, of weighing, etc. It must, however, be remembered that even in these elementary descriptive statements the beginning of hypothesis construction must be recognized. The description chiefly serves for the characterization of a state of affairs in a way which will facilitate the recognition of other states of affairs of like kind. The observer will therefore emphasize, in the description of a fact, those sense impressions which seem to him essential and will neglect those which seem inessential. The manner in which a descriptive statement comes about is thus in a certain degree dependent upon the scientific attitude of the observer. This feature shows itself in an enhanced degree when descriptive reports are generalized and a fact is described not as unique but as "typical." Then the statement which is regarded as purely descriptive also takes on the character of a hypothetical statement. It is clear that this holds for all descriptions of processes which are characterized as following a typical course after repeated observations. Statements which express a functional connection between particular concepts—e.g., the statement "Cell respiration stands in a definite, regular connection with temperature"—in themselves exhibit the complete type of a hypothesis belonging to the empirical sciences. The concepts employed in them are defined by means of rules of operation, in which it is unambiguously stated how the practical observations are to be correlated with the concepts. In this way the concepts become constituents of the language of science, and their verbal nature deviates more or less from that of everyday language. For that reason the words are often replaced by symbols which are unambiguously defined by the rules of operation. There is no sharp logical boundary between descriptive and functional statements. In many statements of biology descriptive and functional elements are mixed.

The general logical foundations of the formation of biological concepts and theories need not be discussed here in detail, since they hold for all the natural sciences and are therefore ex-

pounded in detail in other parts of this *Encyclopedia* (Lenzen, 1938; Woodger, 1939; Hempel, 1952).[2] For the same reason the characters with an empirical statement must possess if it is to be given a legitimate place in an empirical science will be only briefly enumerated. Such a statement connects a definite number of concepts in a manner which is free from objection from the formal logical standpoint. These concepts must be defined in a manner which makes their correlation with observable elements of experience possible. If this is done, the statement itself is testable by experience. By the making of an observation or by an experiment the statement can be verified or falsified. By 'verification,' here and in what follows, only confirmation by experience is to be understood, and by 'falsification' the absence of such confirmation. Nevertheless, by the use of these terms the question of the "truth" or truth-content of the empirical sciences is not touched upon. This question has no significance in biology other than that which it has in all other empirical science, and it is treated in other parts of this *Encyclopedia*. The uses of a statement of empirical science can also be stated in the following way: On the basis of such a statement, predictions about what is observable can be made. The occurrence of the predicted experience then means the verification of the statement, its absence the falsification of it. A correctly formulated hypothesis therefore has heuristic value in the development of science. On the basis of such a hypothesis new observations can be made, new experiments set up, which lead to the confirmation or falsification of the hypothesis. Hypothetical formulations with which observable processes, in the course of time, can be correlated and according to which such processes can therefore be predicted are called "rules" or "laws," e.g., Mendel's rules of inheritance. This terminology is also often chosen for formulations in which a relation between simultaneously existing observable elements is given, e.g., the law of the constancy of the chromosome number in species. The use of the word 'law' or 'rule' has often led to misunderstandings with which we shall be occupied later. This applies especially to the expression 'nat-

2. See Selected Bibliography at end for full data.

ural law,' now rarely used in biology. In principle we are free to construct hypotheses as we like—i.e., every formation and combination of concepts which satisfies the requirements sketched above is admissible. Psychologically speaking, the imagination of the investigator plays a great part in the conceiving of hypotheses and is frequently called "intuition." In practice, the conceiving of hypotheses chiefly takes place against a background of the state of the science already reached, having regard to the complex of statements which have already become more or less fixed constituents of this science.

Statements about biological facts which according to their structure could be testable by experience but which for practical reasons cannot at the time, or perhaps ever, be tested are admissible as hypotheses even though no judgment about their confirmation is possible. Statements which according to their structure are not testable by experience and which, on account of the absence of an unambiguous correlation between the symbols and the observable facts, can never in principle be verified or falsified have no function in the system of an empirical science. They are neither false nor correct in the sense of empirical science but cannot be used in science and have no legitimate place there. Whether they are, in general, scientifically meaningless is a question which will not be discussed here. But there is still another type of statement whose confirmation is in principle untestable. These are statements which, although externally they have the form of a hypothesis of the empirical sciences, are in fact of such a nature that the rules of correlation between the concepts used in the statement and what is observable constitute the whole content of the statement itself. These statements are occupied exclusively with repeating the rules of operation. These are tautologous statements. If in the statement "The positive phototactic reaction of a *Euglena* is proportional to its light-requirement" the concept "light-requirement" is only testably defined by means of the establishment of the behavior under the stimulus of light, this is a tautologous statement of the above kind. Such tautologies have no direct value for science. Nevertheless, as will be discussed

later, tautologous statements frequently occur in scientific expositions.

If a hypothesis is falsified by a reliable observation, it is abandoned. If it is confirmed by the observation, it is used further in science. A hypothesis which is frequently verified comes to be regarded more and more as a stable constituent of the science and often becomes a habit of thought. If a hypothesis can be tested in various ways in accordance with the rules of correlation and if all these tests verify it, then the hypothesis is regarded as an especially well-confirmed one. If the incorporation of a new hypothesis in the permanent structure of the science takes place without contradiction, this is regarded as a proof of the "correctness" of the hypothesis or of the "truth" of its content. If the incorporation does not succeed without contradiction, then, by a thorough logical analysis of the contradictory statements and their elements, we must investigate whether the contradiction is not merely apparent and whether it cannot be removed by a logical change. In other cases the contradiction can be bridged over by means of accessory hypotheses which restrict or extend the validity of the hypothesis and which, in turn, must satisfy the requirements of testability by experience. Naturally, they must not be introduced only *ad hoc*, i.e., they must not be merely tautological or formulated without any connection with the rest of the system of the relevant science, because in that case they would in principle be removed from any testability. On account of their heuristic value they can often give birth to a new development in science. If a far-reaching contradiction persists between old and new hypotheses, this leads to a "crisis" in the empirical science concerned. This, in turn, leads to a revision of the system of statements hitherto in use. It may then prove to be the case that certain theories that have hitherto been accepted must be abandoned because they no longer correspond to the augmented experience or that the old theory represents a special case of the more comprehensive, newer theory. Growing experience leads to an enrichment of our stock of scientific possessions and necessitates a perpetual revision of the complex of statements of the science, and in this lies

its progress. The contradiction is never in the experience but always only in our formulations. Empirical science knows no "aporia" in the philosophical sense.

If, by the observation of a phenomenon, an existing hypothesis is verified, we say that the phenomenon is "explained" by the hypothesis, especially when we have to do with a frequently confirmed hypothesis. The "explanatory value" of a hypothesis seems to us to be especially great if many different experiences can be correlated with it and if it can be added to the whole edifice of science without contradiction. Some statements are formulated or interpreted with the intention of making a process or a complex of relations "intuitable" or "picturable" to human beings. Many formulations of natural science, to which a high explanatory value is ascribed, are so abstract that this feature of intuitability is entirely absent. The difficulties and misunderstandings which can result in science from the human need for intuitability and from the confusion of "explanation," in the sense here defined, with "intuitability" will be discussed later with examples.

In the following pages an attempt will be made to show that the statements of biology in its various subdivisions satisfy the requirements described above and that biology can be regarded as a system of such statements. According to the way of considering and of investigating biological data, we shall here carry out a twofold division into elementary and complex points of view. This subdivision is purely practical and is intended to make our methodological exposition easier.

B. The Elementary Points of View

In considering an organism, we can direct our attention, according to our inclination and aim, either to its visible peculiarities or to its behavior and performances or to its significance as a member of a reproductive chain, and in accordance with these possibilities it is customary to distinguish the subdivisions of morphology, physiology, and genetics. The individual investigator will, according to his gifts, his training, or his particular problem, direct himself exclusively or predominantly to the one

or the other point of view. But, in spite of the necessarily high specialization of the methods involved, it is naturally impossible for him to devote himself exclusively to the one or the other point of view, so that here also the boundaries are not sharp. In dealing with complex problems, the synthesis of the various points of view is even unconditionally necessary.

1. The Morphological Point of View

The enormous multiplicity exhibited by living things necessitates at the outset a description of the visible "characters" of particular organisms—their forms, proportions, colors, and measurable magnitudes. This morphological description relates not only to the external, but also, in the form of anatomy, to the internal, structures, the construction and mutual spatial relations of the organs. It continues right down to a morphology of the tissues, of the cells and their parts, in the branches of histology and descriptive cytology. An essential feature of descriptive morphology, in the widest sense of the word, is the discovery that with its help a classification of organisms is possible, that their recognition on the basis of a sufficient description is possible. The process of classifying in this way involves at the same time the setting-up of a hypothesis—namely, the conception of the "type" of the organism concerned, by means of which we can undertake, in the particular case, to correlate the objects found in nature with the appropriate type in a manner sufficient for practical purposes.

To begin with, the expression 'type' is to be understood in the widest sense of the word. The process of setting up types occurs in every morphological as well as physiological classification, for the reason that the descriptive elements used for the purpose never merely represent the description of an observed result but are constructed from a series of such results by abstraction of the features which are common and which seem essential to the investigator. Where the process of classification into types is carried out in a deliberately critical spirit by the investigator—say, in connection with genetical or biometrical problems—certain conventional rules are set up for carrying it out. For ex-

ample, a classificatory measure of size is given as the mean value of a definite number of measurements, with its mean error and the width of variation of the character given by the standard deviation. A pattern of markings is arranged according to a series of standardized categories, and so on. But such a strict interpretation of the method of classifying into types is usually avoided, and we content ourselves with specifying such characters as will satisfy practical requirements. The most diverse categories which are descriptively specifiable can be the object of the typifying process. It may be a race, a species, a higher classification unit, a group of organisms brought together from some standpoint other than the classificatory (e.g., all water plants), which is described and so made an object of the typifying process. The species concept of taxonomy is thus only a special case for the delimitation of which, as will be discussed later, other than classificatory notions are required. But an organ, a tissue, a cell form, a metabolic process, a mode of instinctive behavior, etc., can all be objects of the typifying process and in this way undergo a classification. Not only do we work in this way in the morphological branches of biology, but the description of physiological processes on a comparative basis leads to a classification by a like process.

The establishment of the "type" of a form, of a mode of behavior, or of a process involves the formation of a hypothesis, in so far as an arbitrary selection of results of observation is compared and emphasized as essential for classification, and thereby the hypothesis of a regular, common repetition of these characters suitable for classification is set up. In this way a law of coexistence of characters, which is empirically testable, is asserted. To the comparison of results of observation there is, of course, added, in this procedure, an idealization, brought about by the fact that the choice of what is described is to some extent arbitrary. This idealization appears in increased measure in the working method of comparative morphology when this proceeds to the setting-up of higher types of higher order. By the comparison of types of similarly organized living beings, we reach in comparative morphology the highly abstract concept of the

"constructional plan." In this comparison similarities and differences are established, and essential characters are distinguished from inessential ones; and in this way a "fundamental" constructional plan, common to several organic types or a large group of organisms, is constructed. By further comparisons more and more comprehensive, more general plans of construction can be set up. The bodily parts, organs, and parts of organs which correspond to one another in the constructional plan are called "homologous" structures.

By comparing the structural plan, say, of a vertebrate, with the concrete natural objects, e.g., with a horse, a whale, and a bird, the procedure of "homologizing" is carried out. The anterior pair of legs of the horse is homologous with the anterior fins of the whale and with the wings of the bird. Homologous organs are thus such as correspond to one another in their place in the structural plan, those which can take the place of one another in the structural plan and which coincide in the plan of higher order. Homologous organs exhibit a construction out of fundamental elements which correspond to one another—e.g., homologous parts of the skeleton, of the musculature, of the innervation.

The formulation of the concept of homology has the typical form of a hypothesis of empirical science. It represents a system of schemata, the structural plan with the spatial relations of characters and organs which are characteristic of it, and it can be empirically tested in so far as the presence of the homologous organs can be verified in every concrete organism by a morphological investigation. It permits predictions of the kind which assert that, if an organism shows itself to be a vertebrate by one characteristic feature, then we may also expect the other homologous structures of this group. Since the domain of validity of the hypothesis is restricted to a particular group of organisms, we should not, in the case of a falsification, place the organism under investigation in this group, i.e., the Vertebrata. It will "correspond" to the structural plan of another group.

In spite of the high degree of idealization which is involved in the setting-up of structural plans of high order and in an ex-

tended use of the concept of homology, the statements of comparative anatomy are nevertheless empirical statements. The elements for the construction of structural plans are derived from experience, and the concepts used for this purpose remain, through their definitions, empirically testable. In this way the extreme abstractions of morphology, in so far as they remain scientifically useful, are distinguished from the entities of geometry, the construction of which takes place on the basis of a purely intellectual conception and for which the requirement of testability by experience is meaningless.

As an example of a morphological statement of wide scope, we may mention the law of the constancy of the chromosome number for any species. It holds for all organisms (with the exception of microörganisms which have no nucleus) and states that in all mitoses of a member of a group defined as a species (we shall return to this definition later) the same number of chromosomes is always to be observed. Instances of the falsification of this rule require the setting-up of testable accessory hypotheses, e.g., the hypotheses of chromosome races, of polyploid races, and the like.

Statements like the above are reached by a process which M. Hartmann (1948) calls "generalizing induction." The verification of a statement by an experiment he calls the method of "exact induction." These expressions do not seem to me to be well chosen, since an essential methodological distinction between the two procedures cannot be maintained on closer examination. It would, perhaps, be better to contrast the two procedures as "comparative" and "experimental." It is by no means the case that obtaining and testing statements by comparison is especially characteristic of the morphological point of view, let alone the only one open to it. A large part of morphology is called "experimental" morphology because the method of obtaining and testing statements is that of experiment, and thus "exact induction." The statement "A certain plant forms finely divided leaves under water but on the surface has undivided floating leaves" can be tested not only by observations on cases occurring in nature but also by experiment, as by arti-

ficially setting up the conditions mentioned and seeing whether the predicted connection is realized. This statement connects concepts descriptive of the environment with morphological concepts.

There is a tendency to assign to experimentally testable statements a greater "demonstrative power," or at least a greater picturability, than is assigned to statements which are testable by comparison. It is customary to contrast them, as "causal-analytical" statements, with "systematic" or "order-analytical" statements. The expressions 'causal-analytical' and 'order-analytical' are not very fortunately chosen. In the first place, it is a question not of analytical statements in the logical sense but of synthetic ones. Moreover, neither a causal connection nor an ordering is "analyzed." It would be more correct to say that these statements set up rules or laws according to which in the first case a succession of states in time, in the other case a co-existence of characteristics or a correlation between properties which is empirically testable, is predicted. We could—with certain reservations—also speak of "the search for a causality of process" or of "a search for an order." If we wish to restrict the concept of "order" to the establishment of a law of coexistence of properties, then only those statements are to be called "order-analytical" which are formed on the basis of repeated observation of such a coexistence and are formulated so as to be testable on further observations of the same kind: for example, statements about anatomical structures or about the results of bio-chemical analyses. If a statement is formulated on the basis of repeated observations of processes in which one particular order passes regularly into another, then this statement is also obtained by comparison and is testable by further comparative observations. Nevertheless, in such cases it is customary to speak of "causality of process," and we should have to regard these statements as causal-analytical ones, although they have been obtained by comparison. Experiment is then a special case of this procedure in which we deliberately bring about a particular initial situation in order to test the succession of changes of state demanded by the hypothesis. By arbitrarily varying the

initial situation, experiment opens up new possibilities for the setting-up and testing of hypotheses and in that way leads to the enrichment of science. Since the expressions 'causal-analytical' and 'order-analytical' are much used in the literature, they will be retained in the following pages, in spite of the reservations just mentioned.

Both types of statements are genuine statements of empirical science if they satisfy the requirements sketched in Chapter I. The changes of state in dead, and especially in living, nature to which our experiences relate bring it about that we are often placed under the necessity of transforming order-analytical statements into causal-analytical ones. In this way one established order is traced back to another, and a rule is set up for the transformation of one arrangement into another which is empirically testable. It is a question of great importance from the point of view of the theory of knowledge whether, in principle—if only under certain, perhaps not always given, assumptions—*every* order-analytical statement can be transformed into a causal-analytical formulation. The question then arises about the "final order" to which everything must be referred back. Since this and similar fundamental epistemological questions are common to all empirical sciences, and biology occupies no special position in relation to them, they will not be discussed here. In any case, it very often happens in biology that order-analytical statements are transformed into causal-analytical ones and that attempting to do this is of great heuristic value.

In the field of morphology this is the case when questions of form and structural plan are expressed as questions of formation and development, in the ontogenetic or phylogenetic sense. M. Hartmann (1948) has expressed the opinion that the "exact inductive" method is superior to the "generalizing inductive" method and, by way of example, compares the following statements: (1) "All carnivores have a relatively short, all herbivores a relatively long, gut" (generalizing inductive); (2) "On an animal diet tadpoles acquire a relatively short gut; on a vegetable diet they acquire a relatively long one" (exact inductive). The second statement, which can at any time be verified by experi-

ment, carries more conviction, according to Hartmann, than the first. W. Zimmermann (1948) on the other hand, objects that the example is not well chosen, because, for testing the efficiency of the two methods, two very nonequivalent facts are compared. In the case of the tadpole we are dealing with an ontogenetic developmental process which is dependent upon diet; the example from comparative anatomy has a scientific meaning only if it is formulated as a phylogenetic question. The assumption must then be made that, in adaptation to dietetic conditions, among the various animal species the different relative lengths of gut have in the course of phylogeny become species-specific, genetically fixed characters. I agree with Zimmermann that in the examples chosen by Hartmann we have to do with very different facts, but I am not of his opinion that the first statement, which belongs to comparative anatomy, must be denied all scientific value, inasmuch as it is not formulated as a phylogenetic statement. As a pure order-analytical statement, it satisfies the general requirements of an empirical statement and at the same time calls attention to an existing regularity. If we wish to transform it into a phylogenetic statement, then we assume that similar statements can be formulated about all processes occurring in phylogeny and that these would be—at least in principle—empirically testable. But then the statement is transformed from an order-analytical one into a series of causal-analytical statements.

A greater measure of picturability attaches to the concepts and formulations of morphology than is the case for the statements of other branches of biology. In using these concepts, people are often tempted, in consequence of this picturability, to choose a linguistic expression which, although very picturable, nevertheless does not, strictly speaking, agree at all with the logical foundations of empirical science. This is so, for example, when it is said that in the development of an organism a structural plan is "realized," that the development "deviates from the structural plan," or that it "follows the structural plan" or "is governed by it." The dubiousness of this mode of expression becomes still clearer when we speak in comparative morphology

of the "metamorphosis of homologous organs," of a "transformation of shoots into thorns," or of a "transformation of the various biting mouth parts of one insect into the corresponding differently formed sucking mouth parts of another." Everyone who is scientifically educated naturally knows that such linguistic expressions are intended only in a metaphorical sense. The "structural plan" exists, indeed, only as an idea in our consciousness, and the "metamorphosis" of homologous organs is also only an imaginary process. The case is somewhat different when we speak of "ontogenetic" or "phylogenetic" developmental processes, but then other rules hold concerning the correlation with the facts of experience. In the picturability of the notions often used in the discussion of morphological problems there lies a danger of falling into a realism of ideas which, as a legacy of the traditional philosophy of idealistic morphology, has played a great part in the historical development of biology. Some modern biologists do not seem to be quite consistent in avoiding this danger, and some profess quite openly a more or less concealed realism of ideas (see Chap. III, B, 2–3).

There is no sharp delimitation between the morphological and the other points of view in biology. The simplest descriptive data often point to the function of the organ or other physiological relations. Every anatomy, especially every histology and cytology, is in some measure functional anatomy and so leads into physiology or forms its foundation. The morphological point of view has special relations to the problems belonging to systematics and evolution which we shall discuss among the complex points of view.

2. The Physiological Point of View

Physiology is commonly defined as the theory of the functions of the organism, of its organs and tissues, or, better, as the theory of the processes which take place in the organism and between the organism and the environment. Sometimes we are told that only physiology deals with the material and energetic properties of organisms, morphology dealing with their forms.

A statement about physical forms, however, has a meaning for empirical science only if it is testable by experience and hence on material structures. Everything independent of the "material" carries with it the danger of a realism of concepts. If a form or structure concept in a biological statement is defined in a way which is completely independent of the "substance," the whole statement loses its testability by experience and with that its usefulness to empirical science. Other assertions of idealistic morphologists teach that causal-analytical thinking is characteristic of physiology, while for morphology we have "prototype" thinking. Although the notion of "prototype," conceived as a kind of intuitive picture, is not defined so as to be testable in experience, yet idealistic morphologists try to set up a kind of causal relation between the prototype and the empirically testable forms and structures of organisms. We shall return later to these differences of view regarding the concept of cause (Chap. III, B, 3).

It is also incorrect to say that physiology is the domain of "exact induction," while morphology is the field of "generalizing induction." Moreover, many statements of physiology are constructed of purely descriptive elements and are obtained by comparison and thus order-analytically. Again, physiology provides many examples of the possibility of transforming order-analytical statements into causal-analytical ones. Physiology often works with purely physical or chemical methods, as in demonstrating the transformations of materials in metabolism or in the analysis of the processes in semipermeable membranes. It frequently expresses its statements in formulas of a high degree of abstraction in which quantitatively expressible functional relations between two or more variable magnitudes are asserted to hold. On account of this structure such formulations are regarded as especially "exact," as strictly scientific or demonstrative. They are, however, not more and not less demonstrative than every other empirical statement which conforms to the requirements of the logic of science.

The following is chosen as an example of the properties of physiological statements described above: "The respiratory

quotient CO_2/O_2 is approximately 1 in herbivores, somewhat less in omnivores, and still less in carnivores." The descriptive elements of the kind of diet of various organisms and their gas exchange, used in the statement, can be defined by specifying the particular diet and by giving methods for the exact determination of the gas exchange. In this way unambiguous correlation of the statement with the testing facts is made possible. The stipulation about the gas exchange can be formulated quantitatively, as well as the physicochemical composition of the food, if necessary. The whole statement has the character of one obtained by the generalizing inductive method and thus of an order-analytical statement. But, from it, single statements can easily be derived which have the character of experimentally testable causal-analytical statements. This becomes especially clear if we set up more restricted statements about the utilization of nutrient materials in metabolism—for example, about the utilization of carbohydrates, fat, and protein as respiratory material—and in this way connect a whole series of testable causal-analytical statements, which make the rule expressed in the wider statement easily understood. In this way the statement shows itself to be a "blanket statement"[3] about an exceedingly complex occurrence which we can—in this case—analyze quantitatively, and to a great extent, into its partial processes. In consequence of this we regard the process as especially well "explained," since we can, from the more comprehensive statement, derive a diversity of statements which are testable by various methods and which can be connected with one another and with other experiences without contradiction. In this way single statements of a particular kind, e.g., those about the utilization of proteins in metabolism, can be related to more comprehensive statements, e.g., about the respiratory processes.

Let us take another example from the physiology of irritability. The phototactic behavior of *Pandorina*, a free-swimming

3. [The term 'blanket statement' is a translation of *Pauschalaussage*. In correspondence the author explains this term as follows: "By 'Pauschalaussage' I understand a simple statement, the background of which is a complex occurrence, and which can therefore be expressed—under certain circumstances—as a complex of other statements."—EDITORS.]

colony-forming green alga, is characterized by a lower threshold, i.e., the light-intensity at which the first positive phototaxis is observed and below which no reaction occurs; the turning point, i.e., a stronger light-intensity, at which the positive phototaxis turns into the negative; and the upper threshold, i.e., a still stronger light-intensity, at which the negative reaction is also absent. The position of these cardinal points of phototactic behavior is dependent in a definite way upon the hydrogen-ion concentration of the solution in which the colonies live. Here the concepts which are connected in the statement are, on one side, a physical property of the environment (the light of a particular intensity coming from one direction), on the other, a mode of biological behavior (the unilaterally directed movement of the alga colony, which is produced by a particular sequence of strokes of its flagella). The whole statement is thus undoubtedly a blanket statement about a very complex occurrence within the organism, extending from perception of the stimulus up to the response. This blanket statement can also be analyzed into a series of testable single statements, e.g., a statement about the place of perception of the stimulus and its structure, one about the conduction of the stimulus, one about the structure of the flagella and their function, one about the changes of state of the whole reacting system which are connected with the changes of the hydrogen-ion concentration. The light is then called the "stimulus" (or, better, "cause of the stimulus") and the behavior of the organism its "answer" (or response) to the stimulus. The dependence of the position of the cardinal points on the hydrogen-ion concentration is described as a *Stimmungsphänomen* ("phenomenon of mood") and the shifting of the cardinal points according to experimental changes of the hydrogen-ion concentration as *Umstimmung* ("change of mood") of the organism. This simple example from the physiology of irritability illustrates the point that, in formulating statements about such processes, linguistic expressions are frequently used which are taken from the psychology of human experience. In this way statements become more "picturable," without a special "explanatory value" being given to these statements by the

use of such linguistic expressions. The possibility of empirically testing them remains instead just the same as for every other physiological, i.e., empirical scientific, statement. If we wish to define the concept of "mood" (*Stimmung*) in a scientifically useful way, we can do this only by correlation with definite experimental observation, and the same holds for the concepts "stimulus," "stimulus-perception," etc.

The use of anthropomorphic terms from human psychology is widespread, especially in the whole of the physiology of the nervous system and special senses of the higher animal organisms, including man. With a consistent application of fundamental scientific criticism, no difficulties result from the use of these linguistic expressions. Even the complex behavior of higher animals can be treated throughout as a problem in purely empirical science. But that a great danger lies in these anthropomorphic modes of expression, which can lead to a conscious or unconscious psychologizing of biological facts, will be shown later (Chap. III, B, 2).

Only in the case of man does a really new problem arise—from the fact that to him is given a new kind of experience, the inner experience, in the form of his own feelings and sensations. One and the same state of affairs can be described in the form of neurological statements and thus in accordance with empirical science and the requirement of experimental testability, or it can be described as sensation in the expressions of psychology. In this way arises the complex of problems of the correlation of physiological with psychological experiences, the psychophysical problem. Since this cannot be discussed without a fundamental critical analysis of psychology, we shall not deal further with it here. It need only be pointed out that an excellent and unambiguous clarification of the boundaries between the physiological and the psychological points of view from the biological side has been carried out repeatedly in recent times (e.g., Bünning, 1949).

A special role is played by the physiological standpoint in the complex problems of developmental physiology, in ecology, and in the investigation of behavior.

3. The Genetical Point of View

In opposition to the usual scheme, the fundamental point of view of genetics will here be given a separate treatment, since it shows, in practice, a certain independence in biology and plays a part in all complex problems of biology, on account of the great importance of modern genetics. The geneticist does not consider, as does the morphologist, the structure of an organism or those of several distinct organisms comparatively; neither does he, in the first instance, investigate the processes which occur in organisms, as does the physiologist. The geneticist deals comparatively with the individuals of a population which form a continuing reproductive chain.

The word 'heredity' (*Vererbung, l'hérédité*), which is used in genetics in order to denote the reappearance of characters of the ancestors among the descendants, conceals within itself the danger of misunderstandings which even today still often lead to senseless debates. It is taken from the language of everyday life, in which it signifies a process in which an external possession passes from ancestors to descendants. Here we have actual possessors, e.g., father and son, and the inherited object. In a derivative sense we speak in genetics of "inheritance" and of "inheritable properties." The word 'property' is used here as though there were a "bearer," independent of the properties, by which these properties are inherited. Nevertheless, no scientific statement can be formulated in which the notion of the bearer is so defined that it could be tested in experience according to definite rules of correlation, independently of all properties. On the contrary, it is always the properties themselves, and these only, which, in accordance with definitions, can be empirically tested. The organism itself is definable only by means of the properties which serve to characterize it. The idea of a bearer independent of the properties is an analogy with the idea of self among human beings, and its transference to things and organisms is an anthropomorphism, or conceptual realism. In the scientific language of biology, therefore, 'inheritance' and 'inheritable property' mean only that, in the comparison between ancestors and descendants, like or deviating characters can be established for

the classification of the individuals. The comparative method leads to the setting-up of statements of the order-analytical type. To see in the connections so expressed a process of inheritance implies a further hypothesis, through which the order-analytical statements are transformed into causal-analytical ones.

The best-known general statements of genetics are the Mendelian rules or laws of inheritance. In the statement "After crossing a bean with red flowers with one with white flowers, we find (after self-fertilization in the F_1 generation) in the F_2 generation red-flowered and white-flowered plants approximately in the proportions 3:1," there is expressed, in the first place, on the basis of the comparison of the results of repeated experiments, a definite correlation between the characters of the parents and those of the succeeding generations. Owing to the fact that the organisms compared belong to a reproductive chain, the causal-analytical treatment of this relation is appropriate. This was formulated by Mendel in the form of the hypothesis of the hereditary factors. In its later form this is composed of the following statements: "The factor R 'produces' the red, the factor r the white, flower color"; "Every plant possesses a pair of hereditary factors, either RR or rr or [the F_1 hybrid in the above example] Rr"; "Whenever the sex cells are formed, the members of these pairs are separated, and each sex cell possesses only an R or an r"; "Through random combination of the gametes in fertilization, the ratios of the red- and white-flowered plants in the F_2 generation are explained." The assumption that a factor R "produces" the red flower color is conceived after the pattern of the "forces" of physics and, to begin with, means no more than that, in this way, a correlation is proposed between the hypothetical concept "factor" and something observable. If the statement were restricted to this correlation, i.e., had no other content than this rule of operation, then it would be a tautology and therefore not testable by experience. But, owing to the fact that the concept "factor" is invested with still other defining characters which are empirically testable, such as its behavior during gamete formation and fertilization, the whole hypothesis

becomes scientifically useful. It asserts something about hypothetical units which are distributed and about a hypothetical mechanism which distributes them. The heuristic value which the hypothesis possesses is shown by the various experimental arrangements by which it can be tested, e.g., the backcrossing of the F_1 hybrids with the dominant or the recessive parent, the analysis of the F_2 generation by crossing with the recessive grandparent, the testing of the assumption of the purity of the gametes in the hereditary processes in the case of haploid parthenogenesis and haploid organisms, the testing of the mechanism of distribution by tetrad analysis, and so on. Beyond this, the heuristic value of the Mendelian hypothesis has proved itself precisely in those cases which seemed to falsify the hypothesis. The setting-up of testable accessory hypotheses has empirically disclosed new connections and, just through these cases, has led to a significant extension of knowledge. We feel that observations about inheritance are "explained" (for instance, the relative frequencies in the F_2 generation) when they can be derived from the general hypothesis. By combining the methods of hybrid analysis with the cytological and embryological methods of modern genetics, such observations can also be derived from yet other hypotheses, such as those about the minute structure of the chromosomes. Through the consistent combining of all these observations, the explanatory value of the hypotheses involved seems to us to be especially satisfactory.

The Mendelian rules are statements about the relative frequency of the various hereditary types in the generations following a hybridization; they are thus statements about the structure of a collective, i.e., statistical statements. The Mendelian hypothesis of the hereditary factors thus characterizes the distributive processes, assumed in gamete formation and in fertilization, only in a statistical way. Naturally, this does not mean that these processes in the individual case, and consequently the individual hereditary fate of a member of the F_2 generation, are indeterminate—say, in the sense of microphysical uncertainty. All these processes which decide the fate of individual descendants after hybridization are, in fact, definite

macrophysical processes and could be described in a determinate way by other formulations for the individual case, although this would be very difficult in practice. Statistical statements also occur in other ways in genetics, as in the domain of the statistics of variation. All statistical statements require for their empirical testing a treatment of the observational material by the theory of errors. This is a general requirement of scientific method, has nothing to do with the object of the investigation, and therefore does not mean, as some biologists believe, an encroachment of methods not belonging to biology. The frequent use of statistical statements has, especially among some theoretical authors, given rise to remarkable and erroneous judgments about genetics. One observer sees, for example, in the Mendelian rule, because it appears in mathematical form, the ideal example of a biological "law," while another, for the same reason, would deny it every biological character. The lack of reflection from the point of view of the logic of science is especially noticeable in such discussions.

The concept "hereditary factor," which governed the early days of genetics after the rediscovery of the Mendelian rules, has, in the course of the development of this science, merged into the wider concept of the "gene" or "allele." The notion of the gene, as it is used today, is a good example of biological theory and concept formation. Today the gene is defined by so many points of determination that a great number of different testable formulations can be derived from statements in which this concept occurs. In the experiment of hybrid analysis the gene appears as a segregating unit, in cytogenetic experiments as a localization unit, in mutation experiments as a unit of mutation, in embryological experiments as a unit of action. Nevertheless, the concept of the gene is by no means dogmatically frozen, as outsiders sometimes believe, but is in a state of constant change, and a series of heuristically valuable tendencies in modern genetics points the way to a revaluation, perhaps even to a fundamental revision, of this concept.

During recent years the genetical point of view has occupied an increasingly important place in biological research and con-

tinues to gain ground. It seems to be destined to form a connecting link between the other aspects of biology and at the same time to promote a synthesis of biological thinking. It therefore plays a large part in all complex questions of biology, especially in the domain of developmental physiology and, in the form of the genetics of populations, in the questions of systematics and evolution.

C. The Complex Points of View

In this section we shall try to show by means of some examples how statements about complex states of affairs are formulated in biology. Every simple biological experience can be represented in various ways on the basis of the elementary points of view described in the previous section. In the case of more complex situations a synthesis of the various points of view is unavoidably necessary. The subdivision of this section in accordance with some fundamental problems of biology is quite arbitrary and in no way exhaustive.

1. The Organism as an Open System

The attempt to reach more general statements which will be true of organisms on the basis of the various points of view leads, among other things, to the definition of the organism as a system. A system is a structure in which all processes are connected functionally in a more or less complicated way. The rules which assert something about *single* processes occurring in a system then only hold conditionally, with reservations, since in such cases the mutual relations of the processes considered to all other processes are neglected or deliberately simplified. Almost all biological statements struggle with this difficulty. An exhaustive total representation of the mutual relations prevailing in a complex system by means of a single statement is not possible. The more general and comprehensive such a statement is, the more indefinite are the concepts used in it and the less testable are they in experience. That every organism is a system is a very general statement, which must first be supplemented, in every single case, by a whole series of special statements if it is to be testable in experience.

There are systems in nonliving nature also, so that the system character of the organism does not provide the means for a strict delimitation of the living from the nonliving. Naturally, the relations in living systems are for the most part of a much higher degree of complication. Moreover, the nonliving systems are not, in principle, closed systems, although most of them can, in practice, be regarded as such. This is not possible with organisms. On account of the persistent mutual exchange of material and energy with the environment, organisms are open systems to a great extent. All possible changes in the environment can have far-reaching effects in the system. The attempt to treat the living organism experimentally as a closed system leads to its destruction, to death. In consequence of the open-system character it is still more difficult in the case of organisms than it is in nonliving systems to draw a boundary between the system and the environment. Every biological statement, strictly speaking, not only relates to the organism but takes in a part of the environment as well. The "thinghood" of a living being is only a psychological experience of the human observer; in a strictly scientific statement the organism, or one of its parts, occurs never as a completely closed definable "thing" but only as a conventionally marked-off part of what is accessible to empirical science. From the latter point of view it is therefore not meaningful to wish to draw a boundary between organism and environment or to distinguish in principle between living and nonliving parts or substances in the organism. The materials and energies which enter the organism from the environment take part more or less intensively in the processes occurring in it, and the same may be said of those passing out of it. It cannot be meaningfully stated when these chemically or physically definable things are "alive" or when they are not. Naturally, for practical purposes, we can often make a delimitation between organism and environment which is exact enough for the purpose in hand. Similarly, we can often distinguish between living and dead parts of an organism in a way which suffices in practice.

The processes occurring in organisms are so connected with one another that, in spite of the perpetual change of the mate-

rials and energies composing the system and in spite of the most
diverse disturbances and variations of state in the environment,
the actual individuality of the system in question is, within cer-
tain limits, preserved or is restored after being disturbed. Only
if the disturbances by the environment or the processes in the
interior of the organism exceed certain limits is the system ir-
reversibly destroyed. This ability of organisms has been called
"self-regulation." The state in which a living organism finds it-
self is a state of equilibrium (variable within limits which are
characteristic for each organism) in which, in contrast to the
forms of equilibrium prevailing in the practically closed in-
organic systems, a whole series of processes occurs in disequilib-
rium—a state which has therefore been called "dynamic equi-
librium" (Hopkins) or, very appropriately, "flow equilibrium"
(Bertalanffy). Many complexes of biological statements of fun-
damental importance contain more special statements about
equilibrium states or differences of potential of the most diverse
kinds—for example, statements about the function of the en-
zyme system in living cells, about the osmotic relations in cells,
about changes of state in the conduction of nervous impulses in
nerves, and so forth. Such statements need not always owe their
origin to the physiological point of view; statements about
states of equilibrium also play a part in the morphological point
of view.

The most general statements about the state of living systems
are especially concerned with the energy relations in the living
world. The maintenance of the flow equilibrium or, in other
words, the preservation of the potential differences necessarily
present in the living organism, is connected with incessant per-
formance of work; the organism raises in this way the entropy
level of its environment; it feeds, so to speak, on "negative
entropy" (Schrödinger). In connection with such considera-
tions and similar ones, it is often customary to speak of the
"activity" of the organism as one of its special characteristics.
So long as this word is used only to denote the mutual energy
relations between organism and environment, it is meaningful
from the point of view of empirical science. Unfortunately, the

use of such words and ones similar to them often leads more or less consciously to anthropomorphic ideas, like the ideas of active "forces" in physics, which have long been superseded.

In order to make the complicated system relations in organisms more easily intelligible, recourse is often had to so-called "models." In biology experimental arrangements or theoretical constructions are called "models" if they show, with deliberate simplification, one or more far-reaching analogies with the processes in the organic world. As examples, we need only mention the experimental permeability model of Osterhout, the theoretical derivation of the average cell size and of cell division from the data of metabolism by Rashevsky, and finally the molecule representation of the gene. Such models are not hypotheses in the usual sense, since they cannot be directly tested by biological observation in the individual case. Rather, they are complexes of statements which have already been verified by physical or chemical experience and are applied to biological data almost allegorically with conscious restriction and certain reservations. They share with the biological hypotheses the property of heuristic value. The model serves as a general pattern in accordance with which special hypotheses about processes in organisms are constructed which are then directly testable by biological observation. A misunderstanding of this function of models in biology may easily lead to the superficial opinion that through them a series of important properties of organisms is "explained."

Among the various mutual relations both in the structure and in the function of living systems, it is possible to distinguish more or less clearly those of greater from those of lesser importance: a kind of hierarchical order prevails within these mutual relations, which is also called "organization." This feature is also not entirely absent from nonliving systems, yet in living systems it shows a far higher degree of complication. In this general form the statement about the "order" prevailing in living systems has only a very vague scientific meaning. If we give it a purely physical meaning, then it is a statement about the uncommonly high degree of atomistic order which we find in the

structure of an organism in contrast to its environment and which, in fact, occurs nowhere else in nature. If we wish to make use of the statement about the given order for the clarification of biological facts, then we must set up more special statements about the kind of this order, its rules, and their correlation with experience, in order to make it testable by observation. Failure to attend to this requirement often leads to the purely tautological use of general statements about order in the organic world.

It is not only to the whole organism that the notion of the open system can be applied; this notion is also applicable to the parts. This holds especially for the cell, which, even in the union of the many-celled organism, in both its structure and its functions clearly shows the properties of a complex open system and is therefore rightly called an "elementary organism." Since more has become known about the cell nucleus, especially about the highly complicated structure and function of the chromosome apparatus, these structures must also be given the character of complicated systems. The morphological and physiological points of view lead to a complex of statements about the system relations prevailing in the organism among its organs, cells and cell parts, and the processes taking place in them. The standpoint of genetics reveals in the genome a system of special order, which is a guiding center for all these processes and is therefore directive for the establishment of the properties and modes of reaction which can be demonstrated morphologically and physiologically in the organism. The rules for the causal connections between the system of the genome and that of the organism form further complexes of statements of a genetico-embryological point of view. With the high degree of complication which statements acquire in this way, it is understandable that every hasty generalization or simplification jeopardizes the scientific value of such statements and that only the strict requirement of empirical testability guarantees the wider utility of such formulations. For the most part, this requirement is satisfied in the domain of modern genetics in an exemplary manner.

The organism is connected with the environment by a series

of complex mutual actions. If we direct our attention to this, as we do, for example, in ecology or in the study of the behavior of the higher animal organisms, then we consider the organism and the environment, or a particular portion of the environment, as a system. In the biocoenoses which inhabit a particular environment, various organisms stand in regular reciprocal action with one another. In an increased degree this holds for closer relations between different organisms, as these are given in the various forms of parasitism and symbiosis. We can in this way extend the system notion to systems of higher order in which equilibrium states with certain capabilities of self-regulation can likewise be established. What has been concluded above from a consideration of the single organism as a system holds also for statements belonging to these domains of biology in all essential respects.

The high degree of complexity which belongs to the systems of higher or lower order considered in biology brings it about that many of the statements of biology are blanket statements. These establish a regular relation, for example, between the nutrient materials taken in and the end-products of metabolism given out or between the temperature and the formation of a pigment in the body covering or between the moisture of habitat and the occurrence of a particular plant species or between the development of a parasite and the conditions in its intermediate host, and so on. Such statements deliberately refrain from describing in detail all those processes which must be assumed as part-processes in the domain of facts under consideration. They are, nevertheless, typical statements of empirical science. The great frequency of such statements of quite a special kind distinguishes biology from other branches of natural science. Nevertheless, it is not correct to regard only statements of this kind as genuine "biological" statements and to deny this title to statements which deal with single processes in the organism and therefore are or have the type of physical or chemical statements. There are in biology no "more or less biological" statements but only statements belonging to empirical science.

Ways of Work in Biology

2. Growth, Development, Reproduction: The Historical Character of the Organism

It is a peculiarity of living systems that many processes in them, including very essential ones, are irreversible, or run in only one direction. Every organism shows a chain of changes of state following one another in a manner which is characteristic for the organism. From a plant seed, if it does not die, a plant will grow which, if it does not die, will form seeds in its turn. An insect passes from the fertilized egg to the imago through an irreversibly directed metamorphosis, finally forming gametes itself. The fundamental processes of this event in organisms are called "growth and development," by which are understood the most complicated processes—not only the intake of substances but also the differentiation and formation of structures and functions. Moreover, processes are included in a regular manner which lead to the production of new systems of like kind and thus to the reproduction of organisms. These regular changes of state give to the organism a thoroughly historical character, i.e., its present state is essentially dependent upon its previous states and determines its future state. Although this does not distinguish organisms in principle from all other natural objects, yet this historical character is especially clearly seen in them. This rests, on the one hand, on the more or less strong dynamic of the persistent material and energetic exchange of the organism and, on the other hand, on its complex system character, which imposes an irreversible, unidirectional course upon most of the processes concerned. Corresponding to the properties of the organism as an open system, the environment plays a great part in all this as a complex of factors making possible and partly conditioning growth, development, and reproduction. Organisms are normally adapted to their environment in such a way that, in spite of the persistent threat to their existence, a maintenance of the organic forms is guaranteed.

A whole series of the most diverse biological statement complexes deals with the peculiarities of organisms here briefly sketched. What is most striking is the co-ordinated occurrence of temporally ordered processes which lead to the phenomena of

growth and development. The statements in question attempt, especially whenever they go beyond pure description, to set up rules, which are empirically testable, according to which predictions can be made about the temporal succession of growth and developmental processes. Such statements can be set up and tested from a great many different standpoints. We frequently speak of the "problem of development" when we have in mind the developmental processes from the fertilized egg to the complete realization of the structural plan, or other growth processes, from the purely morphological point of view. It may here be once more emphasized that, even with such complicated problems, statements of scientific value are possible within the purely morphological point of view. As an example, the allometric growth equation of Huxley and Tessier may be mentioned. It permits predictions about the relative volume and surface changes of body parts or organs in a living system which changes its form through disproportionate growth of its parts. The validity of this law has been demonstrated for a great number of the most diverse transformations of form which an organism can experience during its ontogenesis. For exceptions, supplementary hypotheses were set up of basically the same structure. It is clear that statements like the allometry law are blanket statements under which a whole series of the most diverse and complicated processes is subsumed. The law implicitly assumes that among these processes such a degree of integration exists that a regular blanket statement is not only possible but actually holds good. This feature gives to such statements in biology their special value.

The germ-layer theory may be mentioned as a further example of a predominantly morphologically oriented statement about developmental processes. In contrast to the previous example, we have here a formulation which does not appear in mathematical clothing but works with descriptive concepts and yet, like the allometry statement, is order-analytical in origin. It gives a scheme for the course of the early development of the most diverse animal organisms and for the fundamental processes of organ formation. It also is a blanket statement about

very complicated processes of growth and differentiation which are represented in it according to precepts from the morphological viewpoint.

Purely morphological statements about questions of growth and development carry with them the danger of losing their empirical scientific character by slipping into a realism of concepts. If we say that a growth process "obeys the allometric equation," we naturally do not mean that this equation is a "force" which pushes the growth in this direction, and that in this way the manner of growth is satisfactorily "explained." When we see the statements of the germ-layer theory confirmed in the ontogeny of an organism and observe the developmental processes by which the "structural plan of the organism is realized," this, again, does not mean that this structural plan and its various developmental phases are present, as it were, as pre-existent molds into which the "stuff" is poured like a plastic mass. Usually only thoughtless formulations of this kind or such as are chosen in an effort to reach better picturability can lead to serious errors in biological thinking.

Morphological concepts can be defined, in a statement which is useful in empirical science and is free from logical objections, only as assertions about structures which are testable in experience, and they cannot occur as independent concepts in abstraction from this testability. The morphological consideration of processes of growth and development which is applicable to the individual case embraces only visible forms of proportions and spatial relations. But these forms are constructed of living cells, cell aggregates, and their secretory products in the course of complicated processes of multiplication, growth, and differentiation of these living systems. All these processes are also accessible to the other elementary standpoints of biology, and only a synthesis of all possible points of view opens up the prospect of penetrating into these most complex connections. This path has been successfully followed, for example, by experimental embryology. Spemann's theory of organizers is a complex of statements which are empirically testable by experimental interference with embryogeny. They say something about the material ac-

tions which issue from certain embryonic regions and about the way in which other embryonic regions react to these actions by growth and differentiation processes. In addition to these, we have statements about the relations between the state of the environment and the developmental processes. The complex interplay of the single processes of embryogeny, governed by organizers of various orders and modifiable by the influence of environment, corresponds with the nature of the organism as a complex open system. The whole physiology of growth and reproduction, the doctrine of hormones and active substances, constitutes a statement complex of a similar kind. In all these statements it is customary to call the magnitudes, measured in experiments, which lie outside the practical surface limits of the organism against the environment the "outer factors," those established within these limits the "inner factors," of growth, of development, etc. But a separation in principle of "outer" and "inner" in such statements is not possible. A hormone which is produced by certain cells of the body is also an "outer" factor for other cells, and the same holds for the action of certain embryonic parts upon the others. Quite frequently, theoretical misunderstandings have arisen through the transformation of the purely conventional significance of the words 'outer' and 'inner' into one of principle.

In a much narrower sense of the word, we can speak of inner factors of development if, turning to the genetical standpoint, we make statements about the part played by the genome as a guiding center of growth and differentiation processes. In an embryological experiment, by operative interference with development, by altering the outer conditions and the like, the present parameters of state of the open system in process of development are changed, and the ensuing changes in the course of development are followed. In a geneticophysiological experiment, by the introduction of a mutated gene, of a genetic defect, and the like, a different state of the whole system is given from the beginning. The typical reaction of the whole system and its parts is altered. Nevertheless, under those changed basic conditions the physiological method of embryological experiment can

still be applied. In this way the possibilities of causal-analytical exploration of the processes are significantly enlarged. Statements can be formulated, for example, in which a gene-conditioned active material is represented as an enzyme for the production of certain pigments. Or the moment in development can be demonstrated in which a gene-determined rhythm of certain cell divisions determines the later form of an organ. Or certain complicated transformations in the metabolism are shown to be a chain of particular gene-conditioned enzyme reactions. Statements of this kind connect the concept of the gene or of its alleles, which is empirically testable by means of hereditary analysis or cytogenetic investigations, with morphological and physiological concepts which are verifiable by observation of structures and by physiological and biochemical investigations. They connect structural and functional properties of the genome with structural and functional properties of the organism and are thus statements about the rules correlating two systems of different order with one another. In this borderland between genetics, physiology, and embryology, biological research reaches the highest degree of integration of its technical possibilities but also the highest degree of heuristic fruitfulness.

3. Organic Diversity and Its Structure

The organisms which live on the earth today appear to us in a very great multiplicity of distinguishable types or forms. This multiplicity is a discontinuous one; the types are not connected with one another by all conceivable transitions. This multiplicity is, in fact, very great, but it is finite; we can determine the number of distinguishable types. This work has been completed, at least for certain groups of the animal and plant kingdoms. The given multiplicity does not admit of an ordering by purely quantitative characters, as is the case in, say, Mendeleev's system of the chemical elements. In the domain of organisms, therefore, we could not predict with certainty the properties of a form at present unknown, as was the case prior to the discovery of still unknown elements. Nevertheless, the given multiplicity of organisms is not completely devoid of or-

der, that is, free from any characteristic which permits us to set up a definite order in the multiplicity. On that account men have, from the earliest times, attempted to set up a system of the animal and of the plant kingdom. Attempts at systematic classification according to some one or a few arbitrarily chosen characters have led to the so-called "artificial" systems, e.g., the Linnean system of the flowering plants. We call our present-day system a "natural" system because, in setting it up, as many characters as possible which have proved to be the essential comparable characters, after a careful comparison of the material, are taken into account. The system is based on the concept of species, which will occupy us later and is, in addition, constructed in accordance with the more or less great morphological, possibly also physiological, similarity of the types. Similar species are united to form a genus, similar genera to form a family, and so on. In the establishment of these higher units the systematist proceeds in such a way that he uses for their characterization those properties which strike him, in a comparison of several like species, genera, etc., as special common properties of all members of the groups concerned and which are specially suited to their delimitation from other groups similarly formed. These properties are then distinguished as the generic, family, or order properties. The higher systematic unit becomes in this way an entity which is defined by a few general characters. These are indeed demonstrable—although often in modified form—among the representatives of all species which belong to the higher unit in question; yet the representatives also have the special characters of the species. It is agreed that the higher systematic units, in the first place, are only categories created by the ordering human understanding. Only the doctrine of evolution attempts to see in them natural units, namely, related groups of species with common ancestry. But, quite apart from this theory, it is very peculiar from the purely systematic standpoint that it is possible to unite species into higher systematic units by means of the same ordering principles in all the various types of living things, in spite of their great diversity of organization. In the great multiplicity of living things an order of a

very peculiar structure seems to prevail, which is not comparable to the multiplicity of inorganic things.

Every classificatory approach is based on the concept of species. The differences of opinion about the definition and use of this concept in biology are indicative of the great difficulties of this group of problems. The concept of species is used in very different senses by different biologists, according to their point of view and the problem in hand. Nevertheless, most biologists are of the opinion that "the species," in contrast to the higher systematic units, is to be regarded as a *primary* natural unit. The contradiction which seems to occur here is in some degree understandable in view of the present state of biological knowledge but still often leads to misunderstandings. The pure systematist understands by 'species' a type, a "species picture," which is defined by the enumeration of a series of morphological, possibly also physiological and ecological, characters. Organisms found in nature can be recognized as "members of this species" for the most part with certainty by comparison with this type species. It is left to the tact of the specialist—schooled by experience—to regard certain deviatory types as varieties or races within a species or as another species or, in particular cases, as an aberration or anomaly. When we say that "within this genus so and so many species have so far been described," we are using the word 'species' in this purely systematic sense. That this use of the species notion has persisted for the practical purposes of determining individual organisms and assigning them a place in the given multiplicity is for us a proof of the finiteness and discontinuity of this multiplicity and of the constancy—at least relatively—of its ordering structure. In the view of the "species" as an elementary unit of just this order there lies a weighty argument for its estimation as a natural unit. Yet, even for this purely systematic construction of the concept of species, it is necessary in many cases to institute investigations in the field or the laboratory which go beyond the limits of the comparative method. The membership of the very different female forms among butterflies with unisexual polymorphism within one and the same species only becomes clear

when their complete fertility with the uniform-appearing males is established. The recognition that drought and rainy-season forms in other butterflies, which look so different, are included in one species takes place only when the natural connection between these generations is ascertained. Here a factor plays a decisive part which is not directly derivable from the comparison of forms—that of the genetic connection.

The "members of a species" usually occupy a continuous habitat in which they form a reproductive community. The requirement of complete fertility within the species is for the most part silently presupposed. Wherever the region of distribution is discontinuous, the virtual fertility between members of a species is taken into account for the delimitation of the concept of species even from the standpoint of the systematist, although not in the first instance. But there are cases in which two groups of organisms, which are distinguishable morphologically either not at all or only with such difficulty that the systematist would at most speak of varieties, are accepted as two species because the two groups of organisms are not fertile with one another and thus form—in spite of a common habitat—two completely separate reproductive communities. On the other hand, there are cases in which species which are well distinguished morphologically, ecologically, and geographically can produce completely fertile hybrids. In yet other cases there are, among the members of a systematically unobjectionably defined species, various groups of individuals which are not fertile with one another within their own group but are fertile with the individuals of another group. This is the effect of genetical sterility factors which coexist in the population. The definition of the species as a reproductive community or at least as a potential reproductive community is thus not quite satisfactory, but it at least denotes a real biological unit.

With the inclusion of the criterion of the reproductive community in the species concept, but especially with the application of this concept in every point of view other than the taxonomic, the concept of species changes its meaning and is to be differently defined accordingly. In such statements, for example,

as "This species occupies this or that region, it lives under these particular ecological conditions, it has a particular genic structure," we no longer mean the species type of the systematist but mean instead the totality of the individuals which we customarily include in this species type on the basis of the taxonomic rules and thus a collective of empirically given individual organisms. It is a source of frequent misunderstandings that the word 'species' is used both in the sense of the systematist for the abstraction "species type" and in the other sense for a collective of individuals, but without noticing the diversity of meaning. When we speak of the species as a sum of empirically given individuals, we must specify the point of view according to which this collective is to be marked out and the characters which specify it as a particular structure. The concept of "species" then passes over into the concept of a "population," which will be discussed in the next chapter. It is impossible within the limits of this monograph to enter into the whole complex of problems connected with the concept of species. This much only is established: that in every biological statement in which the notion of species occurs the definition of species must be so formulated that it is unambiguously explained how the content of the statement is testable in experience.

In the drawing-up of natural systems the systematist works chiefly with the methods of comparative morphology and hence according to order-analytical principles. Experience allows him to choose those statements as the most productive which enable the given multiplicity of forms to be ordered in a way which corresponds best with all biological requirements. The use of the concept of homology, its extension to the developmental stages of animals, the knowledge of the essential significance of certain characters (e.g., of the flower structure in plants) for the setting-up of homologous structural plans, and the like, will cause him to introduce a very diverse valuation of the characters of an organism and to build up the system in accordance with these valuations. The system obtained in this way makes use of the concept of the "relationship" of the various living forms. Two species are said to be nearest related when, within the same

genus, they have the greatest number of essential characters in common. In the same sense this concept of relationship is extended to the higher taxonomic units. We are so accustomed today to thinking in evolutionary terms that we often forget that this concept of systematic relationship was originally not meant in the evolutionary sense at all and cannot be used in this sense in pure taxonomy. Without any evolutionary accessory meanings, taxonomy is a scientifically unexceptionable and essential part of biology. It leads to statements about the given organic multiplicity which reveal a series of noteworthy properties of this multiplicity. It is, in the first place, noteworthy that, generally speaking, in the most diverse domains of the organic world groups of species can, according to like ordering principles, be united to form genera and these again to form higher units and that in consequence similar "relationship relations" seem to govern the multiplicity. Further, it is very peculiar that the degree of multiplicity now prevailing in the various domains of the organic world can in no way be predicted according to a general principle or derived from more general laws. While some genera show a great number of species, others are represented by only a single species, and the same holds for the other higher taxonomic units. The potential multiplicity, which we can imagine to ourselves on the basis of what is given in nature as a purely intellectual construction, seems to be realized in the various domains of the world of organisms in very different densities, with very notable gaps or accumulations. Here, also, the multiplicity among living things differs essentially from that in nonliving nature, for the given multiplicity of the elements or of the crystal forms can be derived, more or less satisfactorily, from the more general statements of physics. The purely order-analytical method of taxonomy is not able to undertake such derivations for the multiplicity of living things. The theory of evolution attempts to do this in so far as it transforms the order-analytical statements of taxonomists into causal-analytical ones. In this way the concepts of systematics, for instance, the species concept or the concept of relationship, gain new definitions and new correlations with experience. These new problems will occupy us later (Chap. II, C, 5).

4. The Population as the Natural Form of Existence of Living Beings

In nature there are no species in the systematist's sense but only individuals of various ages, in various stages of development, and in various physiological states, which we speak of as belonging to a particular species—in accordance with the rules of correlation chosen for the definition of the species concept. All together, they form the population, the real form of existence of the species in question at a particular moment of time. Various statements about an organism which are not to hold only for a particular individual and which, moreover, are to be empirically testable in many ways are therefore always statements about the population, about a natural collective. There are various considerations which compel us to formulate collective statements about a population. The mode of adjustment of an organism to its environment; its preservation and successful self-assertion in the environment; the whole complex of ecological, biogeographical, and evolutionary problems requires statements in which not individuals but the natural collective of the population is spoken of and within which the single individuals are distinguished by an average similar norm of reaction. In this way the population is regarded as a system of higher order within which definite relations prevail between the individuals and which, as an open system, stands in mutual relation to the environment. The inner system property of the population becomes especially clear in cases in which the individuals are united into partial systems, accompanied by far-reaching differentiation among themselves, as in the social insects. Also, a united herd as a community of action and a pair in process of reproduction furnish examples of partial systems in this sense. In these and similar cases the whole population is constructed from such more or less firmly united subsystems to form a single supersystem. The population of every organism has its particular structure as a system which can be described by means of a series of empirically testable statements.

The genetical point of view applied to a population likewise reveals its system character. The potential multiplicity of the genome, resulting from the number and spontaneous mutability

of the genes, is realized and distributed in various degrees in the reality of a population. The genetics of populations formulates statements about this genetical multiplicity on the basis of experimental analysis. For the distribution of a mutated allele, for the origin of new allele combinations, for the composition of the population out of such combinations and similar processes, the way in which the reproduction and maintenance of the organism concerned are regulated is of the utmost importance. For organisms without sexual reproduction the rules are quite different from those for organisms with obligatory or facultative sexual reproduction. In the case of obligatory autogamous hermaphrodites (for example, many flowering plants) other presuppositions are given than is the case for organisms with separate sexes or even for those the population of which is composed of groups which are sexually isolated from one another. The genetics of populations regards the organism and its population as a genetical system, the peculiarity of which lies not only in its stock of virtual and actual genetic multiplicity but also in the individuality of the apparatus which serves for the distribution, enhancement, or diminution of this multiplicity.

The relations of the genetical system to the environment are conditioned, above all else, by the testing which the gene-conditioned type of reaction gives to the organisms in their struggle for existence. The concept of the selection value of the various gene combinations is defined, in statements belonging to the genetics of populations, not merely speculatively but in accordance with testability by observation and experiment. The study of the population with respect to its potential and actual fertility and its relations to the environment forms the foundation. Statements about the nature of the reproductive relations are therefore of the greatest importance for all population problems. We must take into account whether the reproduction in a population shows panmixia; to what degree the virtual panmixia is actually realized in the population; whether genetic, sexual, ecological, or geographical isolating factors break up the population into several or few separate reproductive communities; and the like.

Since statements about populations concern a collective, they very often have a statistical character. The mathematical methods of population statistics form the equipment for all biological statements about populations. Making more or less arbitrary assumptions about the initial magnitudes, population statistics formulates, in a purely mathematical way, the consequences which result from the peculiarity of the organism concerned as a reproductive and as a genetical system. These general statistical statements about the waxing and waning of a population, about the increase and decrease of its genetic multiplicity, about the formation of races, etc., are models which, by the substitution of special magnitudes, become transformed into empirically testable hypotheses in the special, individual case. The whole population of an organism is, in its inner system relations and in its relations to the environment, in an equilibrium, which can experience certain fluctuations and which, by analogy with the state of a single organism, can be called a "flow equilibrium" or a "dynamic equilibrium."

5. The History of Organisms

It is not only the consideration of the single organism which compels us to regard the organism as an open system with a historical character (see Chap. II, C, 2). The momentary state of a whole population is also dependent on its previous states and determinative for the future states of the population, subject to perpetual mutual interaction with the environment. If we extend this point of view to the whole organic world, we come necessarily to the total view that the populations of the various living things stand in mutual relation to one another and to the environment and thus, together with the total environment, form a complex system in which dynamic equilibrium prevails, subject to certain fluctuations and displacements. In such a total view the question of the earlier states of the organic world cannot be separated from the question of the history of the earth, which is regarded as a historical object by the remaining branches of natural science.

The question of the history of organisms would therefore have

to be regarded as a genuine problem of empirical science even if we possessed no witnesses concerning earlier states of the world of organisms. But we do possess such witnesses, in the form of fossils which give us a picture, if only an extremely incomplete and defective one, of the diversity of living things which have formerly lived on this earth. The information which these remains give us is especially defective regarding the very long period of the earth's history prior to the Cambrian, during which living things must also have existed. In spite of this defect, paleontology shows us, in a convincing way, *at least* that the multiplicity of organic forms was formerly different from what it is today, that this multiplicity has undergone great changes during the history of the earth, that certain forms of life have died out without leaving any descendants behind and that certain great groups of the plant and animal worlds, which we regard as the most highly organized, have not existed in early epochs. In this way the question of the processes which have taken place in the history of organisms and the investigation of such processes by the methods of empirical science have become essential problems of biology.

The totality of such processes is usually called the "evolution of organisms," and the part of biology concerned with it is called the "doctrine of descent" or "theory of evolution." No subdivision of biology is to such a degree choked up with unrestricted theorizing or fogged by fanciful speculation or made the battleground of extrascientific differences of opinion as this. Especially when the question of the descent of mankind is taken into account, this purely biological problem becomes involved in the arena of philosophical controversies. A clarification of this situation, which is so interesting from the cultural and logical points of view, will be attempted later (Chap. III, C). Here it must be emphatically pointed out that, in the domain of evolution also, only those questions have a legitimate place in scientific biology which are formulated as problems of empirical science. Moreover, the whole complex of evolutionary problems can be treated in biology only in accordance with the principles of empirical science and in this respect is in no way distin-

guished from other branches of biology. Statements about the history of the organic world have, of course, the peculiarity that they relate in part to epochs of the remote past from which no witnesses of the organisms, or only indirectly accessible ones, have come down to us. For that reason a large part of the statements about evolution will forever have the peculiarity of not being empirically testable in practice and will thus consist of hypotheses about the confirmation of which we cannot judge. If these hypotheses are to maintain their place in science, they must, of course, be so formulated as to be *in principle* empirically testable. They then often have the property that, by analogies with practically testable statements, something can be inferred with a certain probability about the degree of confirmation which these hypotheses might have in case there was a practical possibility of testing them. In no case are hypotheses admissible if their structure is such as to preclude the possibility of empirical test *in principle*.

It is customary to treat the whole complex of evolutionary problems in three groups of statements: (1) Statements which assert that evolutionary processes have, in fact, taken place. Such statements seem to be fully verifiable, in view of the paleontological findings. (2) Statements about the way in which the evolution of the existing multiplicity of living things has, in general or in particular, been brought about—thus the assumption, say, of the gradual transformation of one species into another or the assumption of a phylogenetic tree with dichotomous branching and so of a real derivation of several species from a common ancestral form. In a more comprehensive form, such statements set up a common phylogenetic tree for the whole organic world ("phylogeny"). (3) Statements about the processes in living beings, and between them and the environment, which have taken place during evolution and are going on still and confront us phenomenologically in their totality as the evolution of forms. This group of considerations is often referred to as the question of the "forces, causes, factors, or mechanisms" of evolution. Such formulations conceal within themselves the danger of conceptual realism leading to misinterpretation, as

though there were certain scientifically accessible realities, in addition to the processes, in the organism or between this and the environment which "guide" the events of evolution, just as in an obsolete, primitive view of physics the "forces" push and pull the inert particles of matter. For formulating statements belonging to groups 2 and 3 and for their empirical testing, the following possible points of view especially are available: (1) the paleontological; (2) the biogeographical; (3) the comparative anatomical and comparative embryological; and (4) the genetical, that of the genetics of populations and related experimental methods.

Paleontology contains statements about hypothetical evolutionary series, which can be tested by fossil material. When the material lies in easily datable, uninterrupted strata, statements about limited parts of a phylogenetic tree can be regarded as empirically verifiable, although immediate proofs for the natural connection of the generations and the statistical properties of the populations in their full extent are not obtainable from fossil material. Statements about greater phyletic connections and about the whole phylogenetic tree of organisms can be verified or falsified by the material of paleontology only with greater or lesser probability. Nevertheless, paleontology can set up certain more general hypotheses about phylogeny which, being obtained by the method of generalizing induction, can be tested on fossil material. As examples, we may mention the theory of typogenesis and typostasis of Schindewolf and the so-called "Dollo's law." The paleontological point of view is thus quite capable of setting up empirically testable hypotheses about the presumed course of evolution without the help of other possible points of view. Regarding the processes which have taken place in the organisms and between them and the environment during evolution, paleontology naturally has little to contribute. It is misleading when some paleontologists make use of concepts borrowed from other branches of biology—such as the concept of "mutation"—which are scarcely testable on fossil material.

The point of view of plant and animal geography proceeds

from the distribution of the organic multiplicity prevailing at present in its relations to the inhabitable space, and from the displacements in this distribution which are still unambiguously determinable historically. It formulates hypotheses about phyletic connections which are testable by this distribution. The theory of *Rassenkreise* of Rensch and the theory of race and species formation by geographical isolation, as on islands, are examples of the way in which evolutionary hypotheses are formulated as statements which are testable by biogeographical observations. In its retrospective formulation of questions, this point of view finds its connection with paleontology; in its ecological aspect its affinities are with the statistics of populations and the genetic and other experimental methods. Statements obtained by the biogeographical point of view are naturally restricted to fragments of the total event of phylogeny—mostly to the youngest branchings of the phylogenetic tree—while questions concerning its fundamental structure are beyond the grasp of this method. On the other hand, the statements of biogeography and ecology are often connected with the question of the "causes" of evolution, since they deal with the observed distribution of species and races in their testable connection with the environment.

The comparative anatomical and embryological point of view is that from which the idea of evolution chiefly took its origin and from which even at the present day argumentation takes its starting point. The system obtained purely order-analytically is transformed into a causal-analytical one by the introduction of the idea of evolution. The ideal "relationship" according to greater or smaller similarity is reinterpreted as a narrower or broader real blood-relationship. For that reason the statements of systematics and comparative anatomy become more picturable for the thinking human being, and therein, chiefly, lies the charm of this idea. This is all the more intelligible when we consider that the given multiplicity of recent and fossil organic forms cannot otherwise, in its organization, be reduced to any other rules of empirical science and therefore would seem completely unintelligible without the idea of evolution, i.e., would be

without evident connection with other data of natural science. Statements about the supposed phylogenetic tree or single developmental series, which we owe to the comparative morphological point of view, still clearly betray their order-analytical origin. The systematist distinguishes his species by means of certain characters and brings together in one genus all those species which have certain common characters, which are now called the "generic" characters. In a similar way the comparative anatomical point of view constructs the hypothetical phylogenetic tree by regarding the special characters of the race or species as the phylogenetically youngest, those of the genus as phylogenetically older, and so on. The hypothetical "common ancestral forms" and "missing links" of the doctrine of descent arise from similar order-analytical thinking, and in this abstract form, in which they issue from the comparative method, they naturally cannot correspond in all points with a real living being. A phylogenetic tree constructed in this way consists at first of structural plans and developmental series of an abstract kind, the correlation of which with experience now must take place on the basis of the hypothesis of evolution, according to rules of correlation other than those used in the purely morphological point of view. For the constructed phylogenetic series the real genetic connection must be demonstrated or made probable in experience or must be refuted. This requirement is often overlooked, and statements about "relationship" or about developmental series in the comparative anatomical sense, obtained by purely order-analytical methods, are formulated as statements about real evolutionary processes; in such cases the idea of evolution enters as a kind of habit of thought which is taken for granted. This holds in a high degree for comparative embryological statements, which, in the form of the so-called "biogenetic basic law," were formerly often very uncritically treated. It must nevertheless be emphasized that all statements obtained by the comparative methods have their legitimate place in biology as hypotheses about the evolutionary process, if they are formulated as statements which are testable in principle, and thus even when they are not testable in practice, per-

haps because the fossils can give no information about this part of the process, and the other points of view, such as the biogeographical and the genetical, also offer no point of attack for a test.

Comparative anatomical research chiefly formulates statements about the supposed course of evolution. Through a functional consideration of the anatomical structures, its theoretical procedure is, however, always connected with the question of the evolutionary factors. The concept of "adaptation" comes from the general experience that organisms in their structures and their functions are so well fitted to their specific environment and are accustomed to react to fluctuations in the state of the environment in such a way that, as a rule, their lives as individuals and as populations are to a sufficient extent guaranteed, in spite of the relatively great threat to their existence. The concept is thus ambiguous from the start: it denotes both a state, that of "being adapted," and also certain processes, that of "adapting one's self," here, in the first instance, in the sense of an individual type of reaction. Yet in the doctrine of descent the concept of adaptation is chiefly used in the sense that in the assumed changes of the species type or in the emergence of new species is seen a process of "adaptation" to changing, or new, complexes of environmental conditions; or this process of adaptation is called the "cause" of the evolutionary process. In such formulations we very frequently encounter, in more or less explicit form, errors which are traceable to conceptual realism, which define "adaptation" as an "active principle" and the change of forms as the "effects thereby brought about." Formulations are only free from objections from the scientific standpoint if regular connections are hypothetically expressed by them between the supposed changes of form and the states of being adapted to the environment, in such a way that they are in principle testable in experience. The attempt to make such formulations picturable very often misleads us into representing evolution as though it had led out of a state of not being adapted, or of being ill adapted, into one of being well adapted. From this there result paradoxical discussions such as that of

how, in that case, such a complicated organ as the eye could come into existence by virtue of nothing but small steps of adaptation, when it can only perform its function properly in its completely developed state. But we have no right to assume that there have ever existed nonadapted or ill-adapted organisms, for these would not have been viable. Even the most simply organized living things are ideally adapted to *their* specific environment. Types in which the new adaptation to a changed environment has not been sufficiently successful have obviously died out. In a correct formulation we have to do with the assumption of processes in evolution through which particular adaptations have passed over into other adaptations and thus not properly with adaptation but only with changes in adaptation. The same care in the use of concepts is called for when we speak of "higher development," of "perfecting," or of "progress in evolution." Admittedly, the organization of the higher animals and plants exhibits a higher degree of complication than that of the Protista. Yet this does not mean that the former have acquired a higher degree of adaptation and, with that, of life-efficiency or that we are otherwise justified in introducing a value-judgment. The biological survival value of a species in its proper environment is the only scientifically usable measuring rod.

In dealing with the complex of theories constituting the doctrine of descent, an essential change has come about, since it has become possible, through the progress of genetics, of population genetics, and of related experimental tendencies, to make subdivisions in this field immediately testable either experimentally or by comparative methods. The fundamental assumption of the doctrine of descent presupposes that all those processes which have led during evolution to change in the organic multiplicity in principle also take place today and that the state of this multiplicity at present, as well as every past state at a particular moment of time, is a transition state from a previous to a succeeding one. It must therefore be possible in principle to demonstrate the elements of the evolutionary processes in the organic events of the present day. Those tendencies of research

which deal with these questions on the borders of genetics and evolution formulate, for example, experimentally testable statements about the selective value of genetically conditioned differences, about the genetical differences between geographical races, about the genetical differences between species which form fertile hybrids, about the genetical basis of the sexual isolation of races and species, and so on. Comparative genetics formulates statements about the homologizing of the gene stock of related species and about the properties of organisms as genetical systems with regard to the evolution problem. The inference of the genome—of its mutability, its possibilities of new combinations, its structure as a system, and the possibilities of change in its structural basis—opens up undreamed-of new aspects for the treatment of evolutionary questions in the form of scientific, directly testable statements. Questions about the evolution of organisms are often transformed in this way into questions about the evolution of the genome. Admittedly, they do not thereby become simpler, but they become clearer and become accessible in many respects to experimental testing. But, especially, the evolutionary problems become in this way removed from a purely phenomenological study of the types of organisms and become related to elements of which we have some knowledge that they stand as guiding centers behind the phenomena of organic forms and their modes of reaction and that they possess quite definite, unambiguously definable properties. It is therefore not correct to see in the new synthesis of genetics and evolutionary research only a continuation and a more or less important supplementation of the former comparative morphological method. For evolutionary questions, it is far more important when it is demonstrated that in the genome of species there are homologous genes, the properties and function of which in the genome are very well definable and empirically testable by various methods, than when a homologizing of properties is carried out under the morphological point of view. Only those will fail to recognize this who are accustomed to treat everything that can be empirically established as "properties" of a "bearer" existing inde-

pendently of these properties as a "subject." In this conceptual-realistic view, genes, chromosomal structures, and physiological properties are, just like the peculiarities of form, only "properties" of a species or race, which, from being an abstract entity, becomes the "bearer" of all these properties. Such "species," as well as their supposed phylogenetic changes, are naturally inaccessible to empirical scientific treatment. The individuals or populations whose properties are empirically inferred are scientifically defined only by the totality of these scientifically accessible properties.

Naturally, only questions of race and species formation, of genetic differences between races, species, and genera, are accessible to treatment by the experimental methods just sketched, while these are applicable only to a slight extent or not at all to the question of a genetic connection between the higher systematic groups of organisms. The genetico-experimental point of view restricts its statements, as a rule to the finer branches of the phylogenetic tree, and leaves open many questions about the total course of phylogeny. On the other hand, it offers the great advantage of contributing important statements about processes during evolution and of subjecting the generally applicable questions about the "factors" and "forces" of evolution to a treatment which is free from objections from the point of view of empirical science. From the point of view of method, we often distinguish today between the realm of "microevolution," which is accessible to genetical methods, and that of "macroevolution," which is less accessible to them, without at the same time wishing to attribute to this distinction too great an importance in principle.

It is well known that for a long time two theories under various forms have played a great part in evolutionary research: the theory of natural selection, sometimes rather inappropriately called "Darwinism," and the theory of direct adaptation, or Lamarckism. Most modern biologists, especially under the impress of the development of genetics, support some variant of the selection theory, often called "Neo-Darwinism." Lamarckism has few supporters today, especially on account of the re-

peated failure of attempts to verify special hypotheses derived from it. It may be emphasized that it is quite possible to formulate entirely different views about the origin of the given organic multiplicity as empirical scientific hypotheses, e.g., the assumption of an origin of new species from dead substances or from "nothing." Such hypotheses, however, have little prospect of verification by experience and present great difficulties to a consistent incorporation into existing knowledge. The question of the first origin of living things on the earth can likewise only be formulated as an empirical question within scientific biology. It has led to the setting-up of a series of more or less unobjectionably formulated hypotheses, the verification of which has, however, not yet been possible, partly for practical reasons contained in the hypotheses themselves. This question also has been excessively emphasized for extrascientific reasons and made a topic for polemics—often, unfortunately, in a very unobjective form.

Although the whole complex of problems thrown up by the theory of evolution must in many of its parts always remain in the stage of a hypothesis which is not testable in practice, yet it has done invaluable heuristic service in all branches of biology and has therefore become an indispensable part of the method of biology. In view of the multiplicity of points of view regarding evolutionary questions indicated above, it is not surprising that representatives of the various subdivisions of biology, such as systematists, morphologists, paleontologists, biogeographers, and geneticists often put the problems differently and give the hypotheses a different meaning from their several points of view or estimate their empirical confirmation differently. Differences of opinion which come to light in this way have often originated clarifying discussions and so led to fruitful new efforts. Unfortunately, such conflicts are often unpleasant and fruitless, owing to a lack of understanding of the logic of science. In recent years a clear and far-reaching approximation of the various viewpoints has taken place, and various books, as well as discussions at congresses and symposia, allow us to recognize clearly the development of a new synthesis of all possible points of view.

Moreover, in this most difficult branch of biological investigation a phenomenon has become clear which in many other branches of biology, and in all pure empirical sciences, can be regarded as a touchstone for the fundamental confirmation of the methodical path of these sciences: the spontaneous convergence of all lines of development in the science toward a closed, consistent picture of the world.

III. The Significance of Speculation in Biology

Poesis doctrinae tamquam somnium.—Francis Bacon

To follow the historical development of the methods and concepts of modern natural history from its prescientific sources and to point out the various phases of this development in its interaction with the contemporary spiritual and social situation would carry us beyond the limits of this monograph. Such an investigation would make much in the present state of biology more understandable than would a criticism purely from the methodological point of view. It would also show that the significance of speculative thinking has played a very different role in the development of this science at different times. Under the indefinite and vague blanket label of "speculation" there are brought together here all possible teachings, doctrines, systems, theories, and statements which deal with living things but which do not satisfy those prerequisites which we have learned to regard as fundamental properties of biological hypotheses and theories. Speculative theories are distinguished from the typical form of statements belonging to empirical science chiefly by the great generality of their sentences, by the imprecise definition of the concepts used, by the lack of unambiguous rules of operation for their use, and by various other features which make the empirical testing of these statements, or the derivation of special testable statements from these general theses, impossible. These general statements are often tautological. They frequently deceive on account of their high degree of "picturability" or by

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claiming a higher "explanatory value," or they win the reader by seeming to give a simple common solution to very heterogeneous problems of the most diverse fields of natural science.

The situation in biology at the present day is characterized by the fact that the scientific drive of its various disciplines is almost exclusively oriented in the empirical direction and scarcely takes note of the existence of speculative movements. In the textbooks and manuals, in the numerous periodicals well known to the specialist, we shall look almost in vain for essays of a speculative kind. At the great international conferences in which the latest advances in the biological disciplines are reported and in which, by lively exchange of ideas, the directions for further work are obtained, and in narrowly limited symposia serving to synthesize various research tendencies, speculative ideas are scarcely mentioned. And yet there is a large genus of literature of this kind which is distributed in the form of corpulent books, of brochures, of discussions in philosophical journals, of reviews of philosophical congresses, and of articles in general or popular scientific magazines. They are rarely read by experts and are absent from the specialized libraries, and yet many books having a speculative biological content reach higher sales than the best textbooks. Their readers are chiefly educated laymen, the representatives of neighboring sciences seeking a general orientation, and persons of general education. Through the reading of such works the erroneous opinion is often propagated that "biology" is a peculiar, predominantly speculative science which is antithetical to the empirical sciences of zoölogy, botany, etc., and even that these are subordinate to it. Among the authors of the speculative literature are some philosophical experts who have themselves never worked at biology and often have only a very imperfect and superficial understanding of the biological literature which they have read. But we also often find among them biological experts who have acquired great merit by research but have abandoned it in their later years and devoted themselves wholly to speculation. Another group consists of prominent representatives of biological disciplines, frequently of those belonging to applied biology—

doctors, technicians, and the like—who in their busy lives have found few opportunities for occupying themselves with funda-mental scientific questions but occasionally feel themselves obliged to publish speculative effusions. To follow the psycho-logical roots of these phenomena would be an attractive task but one which lies quite outside the limits of this work.

It is in any case a phenomenon which is so clearly impressed on no other branch of natural science and is especially charac-teristic of biology that alongside the system of expert science such a rich speculative literature should grow up, without there being at the present day a living connection worth mentioning between the two fields. There are a few leading biologists who have undertaken the troublesome task of a critical analysis of speculation from the empiricist standpoint: among them are M. Hartmann, A. Bünning, H. Winterstein, and W. Zimmer-mann. It is certainly no unjustified reproach when the theoriz-ing authors complain of the complete lack of interest in specula-tive ideas on the part of most scientific biologists. Much leading-astray by worthless literature, much conscious or unconscious misuse of science, much baseless spiritual conflict, could have been avoided if more interest in general questions had been shown among empiricists. Many platitudes and crudities of the empirical specialists, many errors of the universalist striving after knowledge, could be done away with, if, in the course of a lively discussion based on considerations from the logic of sci-ence, it could be shown more clearly what the tasks and limits of empirical scientific thought and the prospects for a synthesis with other intellectual possibilities are, especially in the field of biology. In this positive sense the following attempt at a short survey should be judged, in spite of its partly negative criti-cism.

A. The Psychological Function of Speculation in Scientific Biology

It is not to be denied that speculative ideas of a general kind, which cannot be regarded as empirical hypotheses in the proper sense, can have a legitimate function in biology by promoting

the development of this science. There are even some authors who see a danger to the future development of modern biology in its impoverishment of speculative inquiries. It is without doubt the case that in the field of biology, as well as in other fields, the present-day empirical structure of science has gradually developed from a state allied to the philosophy of nature of former times, whereby the speculative views of prescientific and early scientific times have formed the historical, spiritual background. After the European Middle Ages had for the last time offered a world picture of astonishing unity and completeness, we witnessed a gradual dissociation between empirical research and the philosophy of nature. After a period of empirical progress in the sixteenth and seventeenth centuries, there followed in the eighteenth century a time of stronger emphasis on ideas belonging to the philosophy of nature, which exerted their influence far into the nineteenth century and in which are to be sought the roots of many concepts of modern natural science. These concepts have, of course, strongly changed their meaning in the course of development, and many problems and alternatives of that time have become pointless through the progress of empirical scientific work. Nevertheless, it was these concepts that stimulated the research and first gave it direction.

An example of this is furnished by the opposed views of preformation and epigenesis in the ontogeny of organisms, which in various transformations are to be recognized throughout the history of biology. Both have their roots in primitive, prescientific ideas. While the philosophical views of antiquity and of the Aristotelian medieval Scholastics seem rather to have supported the ideas of preformation, the Christian philosophy of Augustinian origin preferred epigenesis. In the eighteenth and nineteenth centuries the two views stood opposed to each other in the form of fundamental biological theories. The present state of knowledge has shown this disjunction to be pointless. These discussions between the two tendencies, carried out chiefly in the field of the philosophy of nature, were often helpful for empirical research by giving it direction but were also sometimes cramping and misleading. In any case, the historical roots of the

concepts of modern genetics are often traceable to the theory of preformation, while the concepts of developmental physiology often have their origin in epigenetic views. The general theory of sexuality of M. Hartmann provides an example from recent times. The author of this monograph has attempted to show that the general statements of this theory are tautologous and do not have the properties of unambiguously defined and heuristically useful hypotheses (1933). Nevertheless, the theory of sexuality of Hartmann has given a stimulus to a great number of valuable experimental investigations which represent an essential enrichment of our knowledge. The special hypothetical starting points of these investigations come, of course, in all cases from the complex of theories of stimulus physiology, developmental physiology, biochemistry, and genetics, and the results accordingly fit into these fields without either proving or disproving the general statements of the theory of sexuality. Their interpretation by means of the ideas of this theory seems only a superfluous decoration. The function of such speculative theories of a general kind obviously consists only in forming a conceptual background offering incentives of a general kind, revealing possibilities to thought, and in this way enlivening the investigation, while the setting-up of working hypotheses always arises from the current state of the science in the form described in the first chapters of this work. The importance of speculation seems to decrease more and more with the progressive increase in knowledge and the perfecting of empirical methods. The author does not venture an opinion on the question whether this process necessarily takes place according to a law of intellectual development.

In this connection the question arises whether there exists a "theoretical biology" as an independent science. Some authors, especially von Bertalanffy have declared themselves firmly in favor of taking this branch into consideration in the organization of teaching and research and have drawn attention to the parallel case of theoretical physics. But I believe that this is not a correct comparison. In the case of physics the mastery of a specific apparatus of applied mathematics presupposes special

gifts and special methods. Yet even here the boundaries between theoretical physics and experimental physics have become very much blurred in recent years and were in part only set up through the misunderstanding of certain physicists about the development of their science. In the field of biology the situation is quite different, inasmuch as here at present no special mathematical apparatus is necessary for the setting-up of a system of theories but, on the contrary, an enduring contact with experimental research. Even the biophysical ideas of Rashevsky, constructed with a great display of mathematics and abstraction, or the apparatus of biostatistics and the mathematical methods of genetics maintain their significance only in continuous connection with experimental research. A deliberate separation of a "theoretical biology" would today mean an intellectual decline or even the encouragement of speculative tendencies which would not promote the development of the science. A *purely* theoretical biology would be unable to make any scientific assertion which would say more than the statements of the special branches to which living things or to which the latter would be subordinate. The concept of "general biology," as it is used in the present work, means nothing more than a synthesis of the single biological disciplines. In this sense it is certainly justified from the teaching point of view but not as a branch of research, since specialization is not then admissible. With the increase of knowledge the integration of the results of the special branches of biology becomes an ever greater, but at the same time a more and more necessary, task. It will best be solved by the working together of experimental biologists who possess the endowments necessary for the task. But the demand for the establishment of a special field of research of "general biology" with its own characteristic methods is not in the least justified.

B. *Parabiology: The Misuse of Speculation*

Under this heading are gathered together speculative tendencies, systems, and statements which in my view either do not exercise at all the legitimate function of speculation described in the previous section or do so only in part or are inclined to sur-

pass the boundaries of this function. This designation is not intended to have a derogatory sense but only to indicate that these tendencies stand "alongside" scientific biology, without showing any scientifically unobjectionable connection with it. That is not to say that these tendencies cannot have other, perhaps very valuable, functions in the life and thought of mankind. For the progress of biological science, however, they seem to be useless, or even harmful in so far as they mislead nonbiologists about the real character of scientific biology and divert the expert from a correct formulation of his problems.

Many fundamental misunderstandings owe their origin to the ambiguity of the word 'life,' by which we denote not only the state of living natural objects as it can be dealt with in empirical science but also the subjective recognition of our existence—and thus introduce a problem of quite another kind, which has nothing to do with the scientific tasks of biology. Even for the expert it is not always easy to avoid these overtones of meaning of the word 'life' when he wishes to use it only in the sense of his science. Connected with this is also the fact that in many parabiological systems an open or concealed conceptual realism is involved in using the word 'life.' In popular expositions we often read: "Whatever new knowledge biological science may have brought to light, what life really is, what its nature may be, we know just as little as formerly; it is, moreover, an unsolved an insoluble problem." In such and similar contexts the concept "life" is personified into a natural object existing independently alongside the living organism. In this sense "biology" then becomes the proper science of this "life," separated from the biology of scientific experts, or having the latter subordinate to it. Some other concepts of biology are also involved in a similar conceptual realism, such as "form," "configuration" ("Gestalt"), "wholeness," "species," "type." That no scientifically useful beginning can be made with such ideas, that they must, on the contrary, always remain sterile speculations, without connection with scientific biology, is indeed self-evident. In what follows, a short survey will be given of various systems of a parabiological kind, without attempting a complete or exhaustive treatment.

The Significance of Speculation in Biology

1. Mechanism and Vitalism

The alleged antithesis between systems of mechanism and systems of vitalism is frequently regarded as a very important and fundamental problem of biology. These two views have appeared in various special forms and modifications and today still have their representatives here and there among scientific biologists. The majority of experts take, of course, a rather detached attitude toward them. On the contrary, these views receive lively discussion in circles interested in the philosophy of nature, in which, today, various kinds of neovitalism find supporters. Regarded from the empiricist standpoint, the mechanistic and vitalistic systems show a fundamental affinity, while the contrasts between them seem to lie more in the formulation. Common to both systems, in the first place, is the view of causality in the sense of an executive causality, while in the empirical science of the present day the causal connection is usually conceived only in the sense of a consecutive causality. Both systems seek for an "explanation" of the organic event by reducing it to a "principle" or a "category" which is "at work" in it, which "causes" it. The concepts of "principle," "category," etc., are reified into "working causes." It is of secondary importance whether the mechanist locates these causes in definite hypothetical "elementary vital units" or in the fundamental properties of "matter" or whether the vitalist seeks them in an "entelechy." There have, indeed, been remarkable combined systems, as, for example, Reinke's theory of "dominants" or the "biodynamic guiding fields" or Pflüger's theory of "life-stuffs" which veil themselves in a physicochemical garb but are essentially vitalistic. On the other hand, H. Driesch has called himself, not without justice, a "consistent mechanist" because his theory presupposes a machine model of the organism and an extreme preformationist view, in which the entelechy "is the engineer who sets this machine in motion."

A further common feature of mechanistic and vitalistic systems is the tautological character of their general statements. When the mechanist ascribes properties to his "biomolecules," when he assumes structures in the fertilized egg from which all

vital and developmental processes necessarily follow, this amounts to no more than a pure tautology, in which what is to be explained is already put into the definition of the concepts serving for explanation. The same is done by the vitalist with the entelechy, which is always required to do what is expected of it for the explanation of the vital and developmental processes. In the statements in which the concept of entelechy or something similar occurs, these concepts are, for the most part, defined only by their connection with the processes which they are to cause. In this way such statements escape empirical testing either by experiment or by observation. It is for this reason that the formulations of mechanism and vitalism have been discussed for many decades on the purely speculative plane, without the great progress meanwhile reached in research having permitted a decision in this controversy. Every new result can be interpreted in the expressions of mechanism or in those of vitalism. There is no *experimentum crucis* which can decide between the opposed standpoints. Naturally, attempts have not been wanting which sought to formulate the statements of the great speculative systems in a manner which is empirically testable or to derive special formulations from the general statements which are empirically testable. The famous examples of the older vitalism—for instance, the experimental separation of cells during cleavage stages—were admittedly proofs against special hypotheses derived from preformationist ideas, such as Weismann's theory of the "genetically unequal" cleavage division, but are not proofs for or against the general theorems of vitalism. General assertions about the "entelechy" or the "forces immanent in the system," for example, avoid giving any information about the difference between the mosaic and the regulative type of cleavage; they cheerfully neglect the question of why the regulative ability has limits and where they are. If from these general statements we try to derive special ones which are testable in experience, then this can only be done by means of the transition to hypothesis formation on the basis of the existing state of empirical research, in accordance with the rules sketched in the earlier chapters of this work. The researches of

Spemann, Hörstadius, Plough, and others have shown, that special, more or less narrow limits are set to the equipotential system and that these limits can be defined by causal analysis of the conditions given in the system or in the environment which are empirically testable. Development takes an atypical course just as consistently and in a manner just as much open to experimental analysis as it does a typical one.

It is characteristic of the newer vitalism that its supporters present a special, perhaps intentional, lack of understanding of the results of genetics and their connection with the achievements of developmental physiology—for example, Woltereck (1931) and Driesch (1944). The classical formulations of the great mechanism-versus-vitalism antithesis are adapted to the state of biology which prevailed at the time of its origin. Like all speculative systems which strive after "final ontological" statements, they cling to the current state of empirical science, which they attempt to petrify in its main features. The results of genetics are therefore as far as possible represented as in principle unimportant or are reinterpreted into "problems of higher order." An idea of genetical developmental physiology—the temporal co-ordination of gene or enzyme action—is emphasized as the problem of "insertion," and this is ascribed to the intervention of the entelechy. Such an assumption could only hold as a true scientific hypothesis if the laws of the relations between the entelechy and its effects were empirically testable by an appropriate definition of the concepts used, under varying experimental conditions. But as soon as we attempt such a formulation, the vitalistic notion of entelechy disappears and gives way to the empirical determinations of the magnitudes of the system under consideration. Driesch, the most consistent thinker among the vitalists, therefore tried in his later works to remove the concept of entelechy more and more from the grasp of empirical scientific methods. While some vitalists would see in the entelechy something "in principle analogous to the forces of inorganic nature," Driesch regarded it as "not energy, not force, not intensity, and not constant" but as something "immaterial, nonspatial." It is not so much this vague definition which makes

the central concept of vitalism scientifically unusable as, rather, the complete lack of hypothesis construction which could contribute to the enlargement of the science by empirical testing. The same is true of the materialistically formulated general basic concepts of mechanistic systems.

A feature common to many parabiological systems is the more or less anthropomorphic character of their formulations, on which, in part, their seemingly explanatory value and their effect on the public depend. As one of many examples, we may mention the considerations which Ballauff (1940) connects with the establishment of structures or processes of a complex kind in the organic world. He holds that in such cases we always speak of "order," but we must always ask where the "orderer" is. A point of view which for every order requires an orderer and for every "law" a lawgiver—some neovitalists say that the role of the entelechy is not "executive" but "legislative"—is typically anthropomorphic, and attempts have been made to describe the vital processes and the behavior of all organisms, right down to the unicellulars, in the language of human psychology. Such an attempt would be perfectly admissible methodologically if only we had the possibility of making statements about the psychic life of organisms which are testable by inner experience in the same way in which statements about the conscious processes of human beings are testable. For that reason extreme psychovitalism is the only form of vitalism which has been thought out thoroughly. That such a procedure is for many reasons not open to biology is clear to everyone, and therefore also the concealed forms of the anthropomorphic point of view must be rejected as inadmissible.

Biologists of the empiricist persuasion are usually called "mechanists" by vitalists. It is objected against them that they attempt to reduce all organic events to the simple physico-chemical laws of inorganic nature, which is held to be impossible. What we are to understand by "reduce" and "the simple physicochemical laws" of inorganic nature is, of course, quite inadequately defined. The scientific biologist is far from beginning work with the preconceived opinion of such a reducibility.

What matters to him is that he should use those exact empirical methods which have proved themselves to be successful in his science. *In principle* these are, of course, the same as those used in the inorganic natural sciences. No biologist will assert that the results of the physics and chemistry of inorganic nature will alone suffice to elucidate scientifically the relations of living things. In organisms we find materials united in the most complex metabolic relations with one another and with the environment in the form of highly complex open systems of peculiar hierarchically ordered structure, such as we do not find in non-living nature. Without knowledge of living things we should never have had examples of such structures, and an essential part of what is given in nature would have remained unknown to us. The laborious scientific exploration of this field has therefore also given rise to sciences with independent special methods within the boundaries of natural science as a whole. Nevertheless, no man doubts that even within organisms the same general physicochemical laws hold as in inorganic nature. The elements show the same reactive properties; the same general kinetic rules hold, and the same thermodynamic relations. Moreover, the often-cited apparent contradiction to the second law of thermodynamics holds only if we falsely regard organisms as closed systems. But it vanishes as soon as we consider the organism with its environment and thus a larger section of nature.

An argument often brought forward from the side of a philosophy of nature is the seeming "improbability" of organisms. We cannot here discuss the ambiguous use of the notion of probability by many authors. In so far as this objection relates to the structure of living things and their energy relations, the matter has been made so clear in recent years from the physiological side (Bünning, 1949) and from the physical side (Schrödinger, 1944) that a general discussion of it in vague terms should be superfluous. Structures which are improbable when considered from the point of view of inorganic models are formed by the special biochemical and biophysical situation in the organism and are thus very probable. The peculiarity of organic events

does not lie in peculiar "forces" which do not occur in inorganic nature but in the exceedingly complicated systematic connection of the materials in their interactions. In view of this organization, the energy changes in the organism are no less "probable" than every other physical occurrence; on the contrary, they are the only probable ones. Peculiar relations prevail in the parts of nature represented by organisms just in so far as the high degree of order (or the thermodynamic potential) becomes greater by an increasing disorder in the rest of nature. Such relations would scarcely be conceivable or predictable from a knowledge of inorganic nature alone. Nevertheless, the experience of inorganic and that of organic science join together to give a unitary and consistent picture of the world, derived from the fundamental unity of method. This characterization of the present-day situation in biology does not signify mechanism in the above sense, still less the acknowledgment of a philosophical doctrine in the sense of materialism.

The attitude taken by biologists toward the great speculative systems is often characterized by some uncertainty and too much aloofness (e.g., Spemann, 1936). There is no point in contesting the doctrines of vitalists and mechanists by factual arguments, as M. Hartmann and L. von Bertalanffy do in so praiseworthy a manner, if the essential point is not emphasized, namely, the methodological defects of these systems. For that reason it is a mistake to adopt the standpoint that, although in the present state of investigation a decision between the alternatives presented by parabiological ideas is not possible, yet such a decision is to be expected of some future state of biology. Empirical science will never make possible a decision about questions which are formulated in a way which excludes treatment by empirical methods. From the point of view of method it is also very precarious to use the concepts of vitalism in order to denote the boundary of the unexplored or unexplorable in an indeterminate manner, without trying to seek a purely epistemological clarification of the question of such a boundary. Again, other biologists are of the opinion that the mechanism-vitalism antithesis is to be overcome by a synthesis to which

both terms are subordinate; yet such attempts easily lead to new formulations of a parabiological kind. Finally, there is a series of speculative theories which, although not openly basing themselves on the systems mentioned, nevertheless have essential features in common with them and, in spite of their often cautious and careful formulations, have a parabiological character. Some of these will be discussed in the following sections.

2. Subjectivity, Activity, Purposiveness, and Conformity to Plan in the Organic World

Some authors believe that living beings are essentially distinguished from inorganic things by their "subjectivity." The notion of "individual" is often linked with this idea. It is clear that the roots of this view lie in the personal experience of human beings, who feel themselves to be indivisible subjects. Closely connected with these ideas is the assertion that organisms are distinguished from the rest of nature by a special "activity." The difficulty of using the notion of "individual" empirically in an unobjectionable way in a physiological view of organisms has already been discussed. According to the level of organization and other circumstances, an "individual" is divisible by the formation of complete new "individuals," or it can regenerate essential parts after loss or support such loss without regeneration. The notion of "individual" is not used in biology in the original sense of the word but only conventionally and must be specially defined for the special case. If this is done, then the notion of "subjectivity" also disappears from special physiological analysis, unless we wish to withhold it, as a vitalistic fiction, from such analysis. The same holds for the "activity" of the organism, in so far as more is meant than a statement complex about the energy exchanges between organism and environment. That the study of behavior and other branches of biology must often use these and similar expressions in its statements in order to make them picturable is self-evident. It will not be difficult for the methodologically educated to estimate the borrowed expressions correctly, serving as these do the interests of picturability, without attaching an ontological significance to the concepts

used in them. This holds especially for the distinction made in various connections between "active" or "living" and "passive" or "dead" parts in organisms or cells. Here, also, such a distinction serves only to illustrate certain physiological facts but has no ontological significance. Analysis always leads only to complicated reciprocal actions of the equally indispensable constituents all contributing to the construction of a highly organized system, but without a special degree of "vitality" or "activity" being ascribed to any one of these materials or structures. It would be a mistake to seek in the organism for special "living" structures, for "centers of activity" or an "active principle" of any kind which makes use of the remaining materials and structures as only the passive "substrate" of its action, as is done in principle in all mechanistic and vitalistic systems. The same holds for such expressions—very often used in biology—as 'potency,' 'primordium,' 'capacity,' 'factor,' 'tendency,' etc. In genuine scientific statements these words never denote independent powers concealed in organisms from which some special activity proceeds but are only illustrative circumlocutions for the assertion of a law or for the definition of a set of conditions which are testable in experience. Although we are again and again compelled by the high degree of complication of organic events deliberately to simplify by considering single causal connections outside their relation to the system, and although it is advisable to use language illustratively in the exposition of such connections, still we must always be aware of the danger of misunderstanding which this involves. Such commonly used notions of scientific biology as "gene," "enzyme," "hormone," "stimulus," "excitation," and the like, are also never quite safe from such misinterpretations and are often used, especially in popular expositions, in a parabiological sense without the author intending, or even being aware of, the fact. It is perhaps more difficult in biology than in the other natural sciences to be popular without being misleading.

The frequently used expressions 'purposiveness' and 'conformity to plan' are also dangerously ambiguous. When we describe the structures of an organism and their functions within

the system and in reciprocal action with the environment, we are naturally at liberty to call all these arrangements and functions "purposeful" in so far as the ability to live and the peculiarity of the system depend on just these arrangements. A system having unpurposeful arrangements is simply not possible under the given circumstances. We can with equal justification ascribe "purposefulness" to every inorganic thing if we regard being as it is as its purpose. It is clear that such a highly complicated open system as a living organism, whose existence is so strongly threatened, must possess a great number of arrangements in order to preserve itself. The fact that in spite of these arrangements a very great number of living things always perish for external reasons, that in the case of certain types of organization every organism perishes at a certain age for internal reasons, shows how difficult it is to preserve these complicated open systems in nature. Moreover, many species of animals and plants can be shown to have died out in the course of the earth's history. The organizations of organisms thus prove to be successful only under particular constellations of external and internal conditions and are purposeful in this sense for the maintenance of the individual and the species. The significance and origin of a purposeful arrangement, such as the elaborately constructed mouth parts of an insect, can be depicted scientifically only from the following three points of view and thereby "explained": (1) its structure and its function and their significance for the preservation of the life of the animal are described; (2) the genetical and embryological conditions of its origin in ontogeny are investigated; (3) as far as this is possible, the processes in the phylogenetical origin of organisms with such arrangements are investigated. The employment of the concept of purposefulness in biology in this sense is perfectly correct. When the question of the function of an organ and of its significance for the survival of the individual or the species is called the question of its "purpose," then this kind of teleological point of view is quite common in biology and has high heuristic value. But it does not *fundamentally* distinguish the procedure of biology from that of other sciences of nature. For even in nonliving

systems it is possible to ask about the significance of the single constituents for the preservation of the system. That it plays such a significantly greater part in biology and is so uncommonly successful heuristically rests only on the fact that living systems exhibit such a high degree of complication. It is part of the definition of a highly complex system that each of its structures and functions—allowing for variations within limits—must be necessary for the maintenance of the system in its particularity and is in this sense purposeful. But in many speculations of a teleological tendency the concept of purposefulness is used, more or less disguised, in that other sense which it is given in everyday language. Here arrangements or actions are called "purposeful" if their aim is imagined by human beings as a goal which is striven after. Extreme finalists seek to "explain" all organic events, even processes of phylogenetic and ontogenetic development, by a "striving for a goal," which they see as a special "principle" which is "at work" in organic things. As in other vitalistic concepts, here also a consistent psychologizing, according to the pattern of human conscious processes, provides the only possibility of making such formulations testable in experience. Later we shall discuss the noteworthy attitude taken by finalists toward the notion of causation.

The situation is similar in the case of certain general statements about the "conformity to plan" or "planned order" in organic beings. A general statement about a persistent order easily becomes tautologous if special testable statements about the rules of this order are not derivable from it. Also, the order-analytical statements of biology, obtained by purely comparative means, possess scientific value only through their quite special content which is testable in experience. Statements about the degree or the height of an order, also, are only useful when they provide a means for measuring this degree. The same holds for the concepts "plan," "constructional plan," "functional pattern," and the like. It is characteristic of many parabiological theories that they turn such concepts into things to which they attribute an action on the "substrate" of organic events. In this way some philosophers of nature speak of an

"antecedence" of the structural plan in the developmental processes in ontogeny. The apparent explanatory value of such ideas lies wholly in the purely anthropomorphic conception which is behind them.

It is not to be denied that the teleological mode of expression makes biological topics very clear and is therefore almost indispensable, especially in teaching biological subjects. Nevertheless, it is doubtful whether the statement "So long as an organic system has not yet reached its greatest possible development, it strives toward it" is to be regarded as a "general principle" of organic development. If such a sentence is not intended in an extreme psychovitalistic sense, it says nothing at all.

3. Wholeness and Causality

In many recent philosophical systems the concept of "wholeness" (*Ganzheit*) plays a central part. Its fundamental assertion is that "the whole is more than the sum of its parts." By many authors the peculiarity of being a whole is regarded as especially characteristic of living things, where types of wholeness of still higher degree are distinguished, e.g., species, biocoenoses, peoples, races, etc. Other authors allow nonliving objects such as atoms also to have this property of wholeness. The principal difficulty for a critical evaluation of such statements lies chiefly in the fact that in them the concepts "the whole," "the parts," and "the sum" are not unambiguously enough defined to be useful in special testable hypotheses. In some of these statements "the whole" signifies no more than our idea of a living thing which we consciously experience as a closed unity, and "the parts" are those ideas of parts into which we divide this whole conceptually according to taste. In such statements the question of the relation between the whole and its parts is a logical or psychological, but not a biological, one. In so far as it is a question of empirically comprehensible parts, or part-processes, in an organism, it is already laid down, in the more or less satisfactory definition of these parts, in what regular relations they stand to other parts of the whole. The word 'sum' in a purely mathematical sense is then never sufficient for a char-

acterization of these relations. Even where we speak, for example, of a summative action of individual processes, a derivative and deliberately simplified mode of expression is being used. Physiological analysis leads us again and again to the complicated mutual actions in which all the constituent parts of a living being are systematically involved with one another and with the environment. It would be a mistake to expect that the processes in an organism could be understood from the sum of all those reactions which its parts would perform if they were not in the union of the living system, for then they would be under quite different sets of conditions and would therefore react quite differently. If the above-stated principle says no more than that in organisms such complicated systems of conditions always prevail, then it is quite justified. Then the notion of "wholeness" is only a paraphrase of the concept of system and is also applicable to systems of higher order, such as species and biocoenoses, as well as to inorganic systems. The same holds for another frequently repeated formulation, namely, that "all processes in an organism are related to the whole." There is nothing objectionable about this if it is only an attempt to characterize the type of a complicated system. But it is necessary, if error is to be avoided, also to take into account the relationship to the environment. In a more special sense the "purposefulness" of the partial processes in an organism is also intended by this statement, i.e., the partial processes run their course in accordance with the maintenance of the whole system (see Chap. II).

Many philosophers of nature wish, of course, to give to the concept of wholeness an explanatory value extending far beyond these limits. This is expressed, for example, in the formulation "The whole determines the parts, not the parts the whole" or "The whole is the cause of the particular course taken by the single processes." Such assertions presuppose that there is another path to the immediate comprehension and definition of the "whole," as well as the analysis of all its structures and partial processes, than the methods of empirical science available to biologists. In making use of the Gestalt experience which

we have in considering a living thing, the morphologist is especially prone to fall into a realism of ideas in the sense of an "intuition of wholeness" or an "experience of a formed totality." It is an age-old need of human beings in considering nature to raise the deep emotional impression which nature and her creatures make upon us into an explanatory principle. Thus we often try to see, behind the forms of living things, especially of the human body, laws which derive their meaning from purely mathematical considerations, aesthetic sensations, or ethical feelings and thus from regions of the inner experience of mankind. The theory of proportions of Albrecht Dürer is one of many examples. But such hypotheses are useful in empirical science only when their elements are testable in outer experience. The concept of "Gestalt" as an immediately comprehended totality has its legitimate place, therefore, only in statements belonging to the study of behavior, to the physiology of the senses, or to experimental psychology, in which the reactions of living things are dealt with. For the more highly organized living things react to all the stimulations in their environment *as if* this were a "Gestalt." Thus, in the statements mentioned, something is expressed about the relations between a particular environment and a particular mode of reaction of the organism which can be tested empirically, with the help of the hypothetical concept "Gestalt." This has nothing to do, however, with the question of in what manner the forms and structures of living things are scientifically accessible in external experience. An idealistic morphology, which tries to adopt toward biology somewhat the same relation that geometry has to physics, will always exhaust itself in tautological assertions. Zimmermann (1938) has called the lack of subject/object separation the fundamental error of this way of thinking and has discussed the arguments against a fundamentally different procedure in the treatment of morphological and physiological questions.

In connection with the discussions of this and of the previous chapter, reference should be made to the various views about the notion of cause which prevail in speculative systems (see also P. Frank, 1932). The teleologists distinguish between "ef-

ficient causes" and "final causes," in which they regard "final causality" (in the sense of aim or purpose causality) as an essential characteristic of fundamental biological processes. A statement formulated according to empirical science—"The state B follows regularly on the state A"—is customarily interpreted in such a way in ordinary language that A is called the "cause" of B. Nevertheless, nothing at all is altered in the statement and its empirical testability if we call B the "cause" of A. Only in the case of human actions is it otherwise, in so far as the state B, intended by the acting human being, is already consciously present as an aimed-at goal; and it is the knowledge of this connection which permits us so to transform the above statement that we include this fact in the definition of the state A. Thus the attempt to make a distinction between efficient and final causes seems meaningless without an anthropomorphic psychologizing of natural processes. Idealistic morphology also seeks to introduce, in its idea of the "archetype" and its relations to development in ontogeny and phylogeny, a kind of final causality into the morphological point of view (cf. Troll, 1948). If rules which are to be testable in experience are set up for the occurrence of an ontogenetic or phylogenetic developmental process, it is again quite irrelevant whether we wish to call the initial state the "cause" of the final state, or vice versa, to call the "form" reached in development the "cause" of the developmental processes. Only in the case of an inadmissible reification of ideas into natural objects and an anthropomorphic reinterpretation of the goal or aim concept does the so-called "archetype" thinking of the morphologist seem to be different from the so-called "causal" thinking of the physiologist. But then the formulation at once loses its usefulness for empirical science. In close connection with the mode of procedure of physiology some authors have believed that they could recognize a special form of causality in "impulse causality" or "catalytical causality" which ostensibly occurs only in certain processes in organisms, for example, processes in stimulus physiology or processes governed by enzymes (cf. Mittasch, 1938). But the peculiarity of the causal process in such occurrences persists only so long as

we isolate partial causes from the event and do not take fully into account, or do not fully know, the constellations of conditions given in the system itself. It is here also, as with the processes supposedly to be explained only by "wholeness," that empirical science makes use of just·such effects, which seem inexplicable by the causal connections hitherto known, in order to discover new parts and new mutual relations in organisms. The use of the methods which have proved themselves in science always leads to a consistent synthesis with the existing knowledge and to an enlargement of the system.

Some developments of speculative biology have concerned themselves especially with the question of the indeterminateness of the processes in organisms. It is clear that many biological laws can be only statistical laws in the sense of classical statistics. These are all statements about collectives and collective processes, like the Mendelian rules of inheritance. Almost all biological processes take place in the macrophysical domain, so that in their case the indeterminateness of the microphysical elementary processes plays no part. This also holds for enzymatic, hormonal, and stimulus-physiological processes, for which an origin from the microphysical domain has often been incorrectly asserted on the basis of superficial estimates. The single exception is the very important process of mutation. The role of the mutation frequency within a population or of the somatic mutative occurrences within a body is therefore only statistically describable. But the action of a mutation when once it has appeared in the life of a cell or of an organism and in their descendants again lies entirely in the macrophysical domain. The idea of P. Jordan of an amplifying mechanism which could allow the processes in the microphysical domain to influence the macrophysical organic event is quite conceivable. But we know no such phenomenon in the behavior of organisms, and it would be difficult to imagine how such effects within an organism, which are only statistically predictable, can be reconciled with its existence as a highly complicated system. The conceptually quite mistaken relation into which Jordan has brought his "acausal amplifier theory" with the question of the human

freedom of will has created for the whole theory an undeserved popularity. Meanwhile, clarification from various directions (e.g., Hartmann, 1948, and Bünning, 1935) renders a discussion of it unnecessary. On the clear conceptual distinction between the fundamental quantum-theoretical indeterminacy (e.g., of a single event of mutation) and the classical statistical indeterminacy (e.g., of the processes of selection) and on the correct evaluation of the great importance of such events for processes on the phylogenetic plane, especially clear ideas have been expressed by Möglich, Rompe, and Timoféeff-Ressovsky (1944).

4. Autonomy or Heteronomy of Living Things: Ontological Questions

Both from the side of the philosophy of nature and from that of empirical biology, well-intended attempts have repeatedly been made in recent years to bridge the chasm which exists between the two ways of thinking. In these efforts attention is often deliberately restricted to the question whether an unambiguous "characterization" of living things is possible without the assumption, at the same time, of ontological statements in the metaphysical sense. Or the question is raised how far a special type of conformity to law could be found in the organic world which would allow a special position to the methods of research in this domain of nature. Moreover, the question has often been regarded as decisive whether these special laws of the living are reducible to the laws which hold in the inorganic world. In this way the question arises which stands at the center of many discussions—the question of the "autonomy or heteronomy" of living things. In many of these discussions there is a lack of unambiguous definitions—for instance, concerning the demands which are to be made of a general "characterization" of living things or of the fundamental "peculiarity" of a law.

Von Bertalanffy, especially, has tried, in numerous weighty writings, beginning on the basis of an extensive biological knowledge, to bridge by means of an "organismic view" the antitheses between the various speculative systems in biology. Supported by the guiding principle of assuming "a special form of law peculiar to living things," he tries to avoid all metaphysics

and is therefore rejected by most vitalists as a mechanist. In his earlier works we find such formulations as "alongside chemical differentiation, still another factor, a special form factor, plays a part." But such an analysis of an organic event into separate processes, divided in accordance with the morphological and physicochemical points of view but without a metaphysical conceptual realism, is meaningless and for that reason can never lead to heuristically useful investigations. The same holds, for example, for the following alternatives set up by von Bertalanffy (1930/31): organizer action is *either* "chemical," conditioned by materials having a formative action, *or* "dynamic," in so far as in the organizer "stronger formative powers are localized" and "an energy gradient exists between it and the environment," in which case it "forces its dynamic" upon its environment, and so "material differentiations are created by the formative movements." If from such statements all vitalistic, executive-causal, and anthropomorphic elements are removed, then these formulations cease to assert anything. Like von Bertalanffy, Ungerer (e.g., 1942) does not wish to be classed as a vitalist but nevertheless uses vitalistically colored statements as guiding principles of a "total or organismic outlook." A noteworthy part is played in the writings of these and other authors by the distinction between "qualitative" and "quantitative" processes, these being opposed to one another as alternatives in the description of formative processes. A distinction is also often made between qualitative and quantitative "causal laws" in biology. But the concepts "quality" and "quantity" have a distinguishing value only for our perceptions of things and not for the scientific reproduction of experiences. It is true that to aid the imagination in predominantly descriptive discussions, especially of a morphological kind, we are accustomed to use the terminology of our "qualitative" ideas; yet we could also reproduce all these facts in a language which, although more prolix, uses only "quantitative" expressions. For a principal distinction or characterization of particular processes or laws, the alternative of "qualitative" or "quantitative" is in any case not useful.

It is interesting that in the course of the development of his organismic view von Bertalanffy has gradually laid aside the vitalistic elements of his theory, so that his characterization of living things today coincides with the view of the organism as a complex open system, which is customary elsewhere in biology. von Bertalanffy has made valuable contributions to the theory of open systems (e.g., 1949, 1950). Ungerer (1942), Alverdes (1939), and some other theoreticians often define the concept of "wholeness," to which they give a central place in their systems, in such a way that it quite, or almost quite, coincides with the biological concept of system. Their demand for a point of view which considers the whole organism is perhaps due to the fact that the unfortunately unavoidable and far-reaching specialization of modern biologists has often led to simplifications and one-sidedness in the representation of biological facts which do not do justice to the system character of living things. This demand is certainly justified. But, unfortunately, behind its residue or kernel there lurks a parabiological view, especially when a general characterization of living things is attempted from a "wholeness" point of view. Living things can certainly be distinguished by a series of peculiarities from nonliving things. This characterization becomes all the clearer the more special the statements become. For a completely satisfactory characterization the whole complex of biological statements must, of course, be invoked. The more we try to make this characterization general, the more the statements become lost in indefinite formulas. The parabiological character of such formulations becomes especially clear when it is sought to distinguish living things from non-living ones by the "introduction" of a single special "force," of a special "principle," or of a special "category." What scientific biology is able to contribute to the characterization of living things will never be able to satisfy the wish of the metaphysician for an "ontological" definition of the living. The assertions of an empirical science are never "last words" or "reality statements" in the sense of ontology. The empirical scientist must decline to reinterpret the theorems of his particular science as ontological ones and thereby to rob them of their proper meaning, as, unfor-

tunately, often happens from the side of the philosophy of nature (cf. Wenzl, 1938). The same holds for the sharp delimitation between living and nonliving or even between animals and plants, again and again demanded by metaphysics, as well as for the special importance which is attributed to the question of the origin of life on the earth. When Troll (1951) devoted much labor and sagacity to deciding whether, "in the view of ontology," the true viruses are living or not, this really only showed that suggestions toward further developments in the empirical sciences were hardly to be obtained from such considerations. Yet these and other boundaries erected in the name of ontology might prove to be unfortunate for this development if we wished to treat them as untouchable obstacles to the methods of research.

The question of a special kind of law for living things, of "laws special to biology," is particularly dear to the heart of many theoreticians. Some satisfy themselves with the general requirement of such laws or with the general reference to the "character of wholeness" of such laws. We have pointed out in our discussions about the various aspects of biology that it is not possible, from the standpoint of methodological criticism, to call some of its statements more and others less "biological." Nevertheless, some biologists are inclined to consider morphological and ecological statements or statements belonging to the study of behavior "biological" statements, but purely physiological or biochemical statements they regard as "nonbiological." Behind such distinctions is concealed the above rejected assumption of "living" and "nonliving" parts of organisms, the assumption of a special specific "activity" in organisms or a mechanistically or vitalistically oriented idea of wholeness. Where concrete examples for "specifically biological laws" are brought forward, we find mentioned, for example, the following: the allometry law, certain laws relating to metabolism, Child's gradient theory, Mendel's rules, certain phylogenetic laws which can be derived from the evaluation of series of fossil forms, and so forth. In all these cases we are dealing with formulations having the type of genuine empirical scientific statements. What

distinguishes them from other biological statements, although of course not sharply, is the fact that they are blanket statements, statements about a very complex occurrence, the initial and final states of which they connect causally by giving a rule. It is clear to everyone that such statements represent the subsumption of a great number of single processes which take place within the system and in reciprocal action with the environment and are causally connected in the most complicated manner with one another and with the environmental constellations. Many of these laws are of the classical statistical type, in which the individual processes, about which they say something collectively, certainly take place on the macrophysical level and according to classical causality. Such blanket statements are not, however, at all confined to biology but occur in every branch of natural science, as in the description of geological processes. The fact that they seem to be especially common in biology depends, indeed, only on the high degree of complication of the processes about which something is to be said. These assertions only preserve their empirical scientific character if we define the concepts occurring in them in a manner which is empirically testable. Statements about "Gestalten" thus only do so if physical structures are intended. But as soon as "form" is viewed in the conceptually realistic sense of idealistic morphology, the statements lose this character. But the same holds also for "form" or "Gestalt" in the statements of inorganic natural sciences and is not therefore the special peculiarity of "specific biological laws."

It is a further question how far blanket statements of this kind can be analyzed into statements about all single processes which take place in the total event, whether they can be reduced to these simpler statements without remainder, or, as is often said, how far the total event can be satisfactorily "explained" in terms of the single processes. It is certainly the case that for none of the examples of biological laws mentioned above has this analysis been carried out completely, nor does the prospect of such an analysis exist. Even the wish for such a *complete* analysis scarcely exists, for it would not be likely to yield any

noteworthy results. Moreover, it would occur to no one to demand of a statement expressing laws of geological processes information about the individual fate of every grain of sand or molecule contributing to the composition of the rocks. It seems much more essential that some important individual processes within such a biological total event should be subject to analysis by the most diverse methods and that the laws thus obtained should not stand in contradiction to the blanket statement about the total event. Where such contradictions have seemed to result, they have always led to the discovery of new, hitherto unknown connections or constellations of conditions. But contradictions *in principle* between blanket statements in biology regarded as "specifically biological" and the statements about the single processes felt perhaps to be "less biological" have nowhere resulted. On this circumstance the scientific picture of the world, which is, on the whole, free from gaps and from contradiction, rests. Naturally, this is not to say that a direct reduction of biological blanket statements to some simple laws or "principles" of inorganic nature can be carried out. Such a requirement fails to recognize the high degree of complication of living nature and arises chiefly from the metaphysical requirement of certain philosophical tendencies. The requirement of an answer in principle to the alternative "autonomy or heteronomy of organic life" is thoroughly metaphysical and cannot be made within empirical science.

It is interesting to note what a philosopher who has a sufficient book knowledge of biology, for example, Ballauff (1943), says about this question. It is striking to an empiricist that in the discussion of biological matters (e.g., the "system problem" or the "Gestalt problem") the most diverse theories are brought forward as co-ordinate, equivalent possibilities for thought or are set up in opposition to one another as alternatives, although some of them are formulations that are free from objection from the standpoint of empirical science, while others are pure metaphysical speculations. In this lack of critical examination there arises a chaos of obscurities, contradictions, and absurdities which has very little to do with scientific biology. Ballauff

himself feels this and, from the philosophical standpoint, applies the sharpest criticisms against the systems which we call "parabiological," especially against their realism of ideas and concepts and the anthropomorphism of their formulations. He proclaims a "gnoseological crisis" in the present-day philosophy of nature. Even biologists like Ungerer (1942), for example, who wish deliberately to exclude metaphysics from their view, are insufficiently critical in drawing the boundary between methodologically unobjectionable theories and parabiological speculation. Ungerer sympathizes, like some other biologists—for example, M. Hartmann—with the "stratigraphical systems" of the philosophy of nature, as they have been developed as a "theory of levels of being," especially by Nicolai Hartmann. Although the careful formulations of this theory approximate closely the form of empirical scientific theories by the utmost avoidance of metaphysical speculations, yet to the empiricist they seem, as general statements, to be suitable only to serve as tautological statements, not as sources for the derivation of special statements which are empirically testable. It seems to be the case that every attempt to build, on the basis of the progress of the special sciences, by way of an "inductive metaphysics," systems which have more to say than the special sciences is condemned to remain a play of ideas which is not able to help the empirical sciences in their work but may only hinder them.

C. World Picture and Philosophy of Life

The restricted and very doubtful function which speculative theories today exercise in the real working of biology, and in the whole structure of the empirical natural sciences, would scarcely justify the efforts which so many thinkers devote to this field and the many interested readers which publications of this sort find. Between the speculative systems of biology and philosophical, literary, religious, and political systems of thought, we find all kinds of transitions. Among these are found systems of high aesthetic charm and of deep ethical content. Philosophical speculation about nature obviously exercises psychological and social functions which lie within the domain of philosophies of life.

The Significance of Speculation in Biology

Many theoretical biologists, for example, Ungerer, Driesch, etc., especially emphasize the importance of biological theory for a world view and from this derive the demand for a stronger emphasis on speculation in biology. The true basis for the growth of speculation thus lies in a need for a philosophy of life.

By 'world picture' (*Weltbild*) and 'philosophy of life' (*Weltanschauung*) we shall here understand well-defined domains of human spiritual life. By 'world picture' will be meant the total representation of the world which we can form on the basis of all statements of empirical science. It seeks to embrace everything which can be accessible to us on the basis of outer experience. The world pictures which various peoples at various times have formed have been very diverse, according to the way of handling empirical methods and the intellectual elaboration of the material of experience. Today, also, the world pictures of different contemporaries are very diverse in content and extent according to the degree of education in empirical science and the spiritual capacity of the individual. A world picture can, in any case, be objectified in principle in a transtemporal and transindividual manner, in so far as it has outer experience as its basis, as a common gift to all men. The world picture is liable to errors and incompletenesses which can be corrected by outer experience. In their practical behavior for the attainment of external goals men guide themselves according to the current world picture. From this practical application of the world picture there result the applied sciences, technology, medicine, agriculture, and the like.

On the other hand, by 'philosophy of life' is to be understood the totality of all emotionally tinged strivings and ideas of a man, the totality of his value-judgments, of his ethical and aesthetic maxims. Ideas about the meaning and the tasks of human life, about behavior toward his neighbors, about the meaning of human societies, and therewith about the whole field of ethical, social, religious, and political activities of a man are rooted in his philosophy of life. While a man thus reaches decisions regarding inner and outer goals of his personal and suprapersonal life on the basis of his philosophy of life, he obtains the

means for the attainment of outer goals from his knowledge of the world.

It is the conviction of the author of this monograph that *every* philosophy of life rests on a faith, on a decision to trust, which can only be reached from an inner human experience. But on the basis of the world picture of empirical science a philosophy of life can never be built. From the statements of empirical science not a single decision in matters concerning a philosophy of life or valuation can be reached. Biology as an empirical science can therefore never give an answer to those "great questions of life" which move men from within. Naturally, this does not mean that occupying one's self with biology or with another empirical science or the mere contemplation of the empirical world picture is not an experience of high ethical and aesthetic content. Yet the sources of valuation or of emotion flow from the domain of the philosophy of life. The synthesis between philosophy of life and world picture, as a spiritual task of every time and of every man, is therefore never only a matter of empirical science but always a matter of faith. The author of this monograph, who counts himself fortunate in having, as a Catholic Christian, a positive belief, has always felt this consistent synthesis to be the greatest fulfilment of the spiritual life. Nevertheless, he believes that the strong emphasis on speculation in the empirical science, and the various philosophical systems of nature found within modern European spiritual history are symptoms of the weakness of faith and the uncertainty and disunity of our times. Many of these systems clearly bear the stamp of religious substitutes. They seek to fill the substance of faith, hollowed out by the Enlightenment and by liberalism, with pseudo-scientific content. In this way elements foreign to science are imposed upon the empirical sciences, and their methodological foundations are threatened.

This becomes especially clear in the great totalitarian ideologies of our time, which have taken over this tendency as a spiritual inheritance of the nineteenth century. In their efforts to subordinate all the religious and philosophical convictions and feelings of mankind to their program, at the same time denying the

latter's foundation in belief and pseudo-religion, they attempt a scientific justification of this program on a basis of philosophy and empirical science. German National Socialism attached great importance to the parabiological "wholeness" theories, because it wished to justify pseudo-scientifically on this basis the totalitarian demands of *Volk* and *Rasse*. Marxism transfers the same mode of argument to "class." Of the biological subdisciplines, genetics, especially, is repeatedly drawn into the conflict of opinions about philosophies of life. While the representatives of exact genetics were called "materialists" by the vitalists, they were branded as "idealists" by the biologists of the school of Lysenko. The philosophical and political ideologues would also like to make their doctrines the foundation and measuring rod for the method of natural science. The author of the present work heard in a programmatic speech of a leading German biologist in the year 1938 these words: "The connection [between "race" and culture] is intuitively grasped by the *Führer*, and it is the one and only task of German science to buttress this intuition scientifically." Similarly, in the condemnation of exact genetics by Lysenko the highest authority was the pronouncement of the party leader, not scientific observation.

IV. Conclusion

In this monograph an attempt has been made to show that biology, in the structure of its statements and in its methods of work, is a purely empirical science. Corresponding to the peculiarity of its object, it has developed special methods and special subdivisions, but these are nevertheless not different in principle from those of other natural sciences. Its assertions, in so far as they exceed the boundaries of pure descriptions, have the form of empirical scientific hypotheses or theories. The explanatory value of its statements rests on foundations similar to those of other natural sciences. The derivative modes of expression or tautological formulations of certain statements which aid

picturability are, corresponding to the peculiarity of its object, perhaps more frequent in biology than in other natural sciences but do not detract from its empirical character.

As an unavoidable consequence of its rich development, biology has experienced an especially marked subdivision into special branches, and this carries with it a certain danger of one-sidedness. The synthesis of the results of biology nevertheless goes on throughout consistently and fruitfully and leads to a constant development of the science. There is in biology no "crisis," as has sometimes unjustly been stated. The synthesis of the results of biology with those of the remaining natural sciences has been fruitfully established in many borderline regions and leads to an empirical world picture which is on the whole consistent and unified, if incomplete.

A critical study of foundations and methods which could only be hinted at in this monograph would certainly be very useful in biology. But here biology occupies no special position, because such problems are common to all of the empirical sciences.

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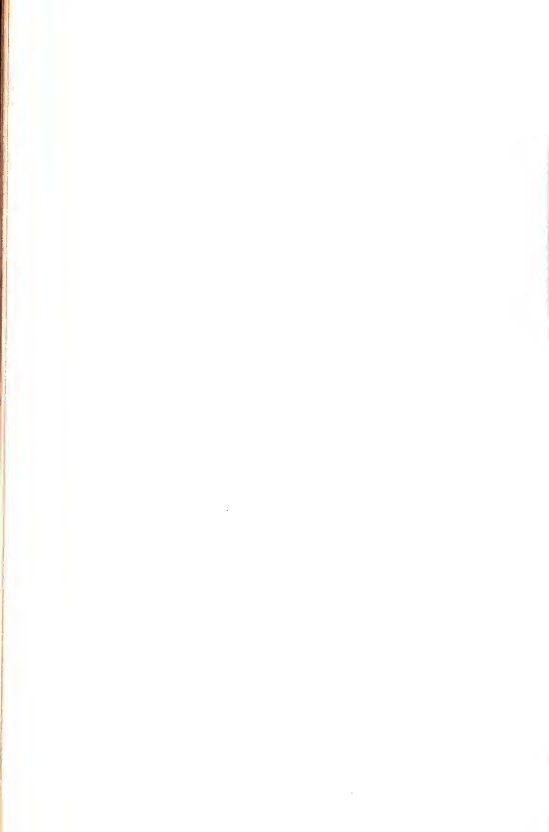
The Conceptual Framework of Psychology

Egon Brunswik

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Contents:

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I. Experience and the Emergence of the Objective Approach

On its way to becoming a science, psychology had to face certain requirements of procedural policy or general methodology.[1] The issues involved fall into two major groups. One deals with the rigor of fact-finding, inference, and communication. In this respect, there must be methodological unity of psychology with the other sciences, especially physics. In contemporary psychological discussion this requirement is often expressed by saying that we must make psychology an "objective" or an "operational" discipline in the general manner attempted by behaviorism (Chap. I). A second set of problems arises in connection with efforts not to lose sight of the specific tasks of psychology in the process of objectifying it but to establish exact study on an adequate level of complexity, sometimes called "molar" or "functional." The thematic identity of psychology can be, and can only be, established by the recognition and programmatic employment of specific research "designs" and aims relatively uncustomary in the other natural sciences. Such diversity is not only compatible with, but necessary within, the basic unity of the sciences (Chap. II).

1. The Primacy of Mind in Philosophical Dualism. Sensationism

Perhaps in no other science is the problem of objectivity as troublesome and as urgent as in psychology. This is primarily due to the fact that psychology uniquely lends itself to entanglement with dualistic metaphysics.

The rationalist answer to the quest for certainty. Introvert

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meditation.—There are two ways in which the "quest for certainty"[1] has been handled in the history of philosophy. One of them, basically subjectivistic, mentalistic, or "introspectionistic" in character, is associated with the rationalistic and idealistic outlook prevalent on the European Continent since Descartes. The other, related to objectivism, is affiliated with the empiricism and positivism of England and America.

The mentalistic concept of certainty is implied in Descartes's *cogito ergo sum* in which our awareness of thought is raised to the ultimate criterion of existence; in the same vein, subjective clarity and distinctness was regarded as the criterion of truth. This was in line with the rationalist tendency to consider reason, and especially mathematical reasoning with its feelings of intuitive self-evidence, as a source of knowledge superior to empirical observation, and of identifying knowledge in general with mathematical knowledge.[2]

The study of conscious experience and the method of introspection thus acquired the stature of an enterprise as worthy and distinguished as the study of matter in physics, if not more so. There existed a challenge to achieve for mind what physics had so successfully achieved for matter. In time, psychology became firmly associated with this task. The subjectivistic conception of psychology remained unchallenged up to the onset of the behavioristic redefinition of psychology at the beginning of the present century. It lent a certain internal coherence to psychology, temporarily lost after the demise of introspectionism as the unchallenged ideology, but about to be re-established in terms of objective features of research design.

Descartes's philosophical method was one of contemplative "meditation," as had been that of Augustine and the mystics. This was introspectionism in the narrower, introvert sense. Aside from the higher forms of cognition, its favorite topics were the "passions" and other complex emotional states. It may be schematically depicted as a relatively static, self-contained form of central encapsulation (Fig. 2, presented later for comparison with subsequent developments).

Sensationism and experiential positivism.—It remained for the English empiricists to shift the introspectionist's attitude

from the so-called higher mental functions to sensation. The peripheral kind of extraversion developed in this process (represented in Fig. 3 by dislocating the reference point of introspection to a position out of center in the direction of the sensory or the motor periphery) became more elaborately cultivated during the first major period of experimental psychology in the second half of the nineteenth century (sec. 15). Upon analysis in "hard introspective labor," conscious data are said to reveal themselves as a bundle of punctiform sensations of color, sound, and the like, including sensory feelings and images; the latter are considered, after the fashion of Hume, as nothing but "faint copies" of sensations. As Titchener has put it: "All the genuine findings of psychology must consist of sensations." For its static, piecemeal character this point of view has been characterized as "structuralism."

The same kind of elementaristic introspection underlies a variety of "impressionistic" manifestations in the late nineteenth century. The most remarkable of these is Mach's conception of sensation as the common starting point for psychology and physics. Its introspective origin is best revealed in Mach's report, in his *Analysis of Sensations*, of a primordial philosophical experience in which "the world suddenly appeared as . . . [a] mass of sensations." According to present gestaltpsychological and psychopathological views, such a position lifts to undeserved prominence a relatively insignificant and uncommon, artificial, distorted, and indirect type of experience.

Mach's philosophical sensationism defines an idealistic position in the tradition of Berkeley's tenet that "to be is to be perceived" (*esse est percipi*). It may be taken as the climactic core of what Boring[50] has recently labeled "experiential positivism," in contradistinction to the "operational positivism" which stresses the objective methods of physical measurement and thus leads to behaviorism rather than to sensationism (sec. 4).

2. Privacy and Limited Communicability in Phenomenological Introspection

From sensationism, two opposite lines of development may be distinguished. One represents a swinging-back of introspec-

tion to more natural and global forms (sec. 2). The other constitutes a direct bridge to the empiricist development of the concept of objectivity in which self-evident "clarity" gives way to cooperational "rigor" (secs. 3 and 4).

Constancy hypothesis and gestalt qualities.—The first of these developments takes place within the modern psychology of form-perception (see sec. 18) and of thinking. The classical notion of sensations as the ultimate building-bricks of perceptual experience is ascribed by Koffka to a "confusion of the phenomenal with the functional." Our knowledge of the anatomy of the sensory surfaces with their mosaic of receptor cells and the bundles of nerve fibers issuing from them, much emphasized in nineteenth-century physiological psychology, is said to have polluted introspection and to have exerted a suggestive influence toward a sensationist interpretation of perception and of consciousness in general. An unrecognized "constancy hypothesis," tacitly taking for granted a one-to-one correspondence of sensory stimulus elements and conscious sensations (represented in Fig. 3 by the solid circle at the periphery and the bracketed broken double arc issuing from it toward the introspective data), is said to underlie sensationism.

Descriptive phenomenology. The "nonpictorial" contents of "imageless thought."—The basic deficiencies of introspection as a scientific tool become evident, more drastically than in perception, in the efforts to establish a descriptive "phenomenology" of thinking. One of the reasons for this is that the lack of a common external reference situation underscores what is known as the "private" rather than "public" character of introspective observation. Related to this is the so-called "noncommunicability" of introspection. There is hardly an instance in introspective psychology in which the helpless solitude of the observer is as clear as in the studies of the so-called Würzburg school.[50] Thinking is in the main described by such negative attributes as 'unanschaulich' (which has been variously translated by 'im-palpable,' 'in-tangible,' 'non-visualizable,' 'non-sensory,' 'non-pictorial,' or 'image-*less*').

An illustration of the artistic character of the language that alone might be able to convey some of the flavor of what was to be communicated is Bühler's term ' "aha"-experience,' used to describe sudden flashes of insight; a phenomenologist outside the Würzburg group, William James, spoke of "feelings of 'but' " in characterizing the frequent spontaneous self-criticism of subjects in thought experiments. Picturesque language of this kind merely appeals to the "empathy" of a richly experiencing subject, to a kind of introspection by proxy as elicited—but by no means precisely transmitted—by the report.

There were elementarist counterclaims such as Titchener's recourse to sensations of kinesthesia as the true vehicles of thought. Woodworth, for a while sympathetic to the concept of imageless thought, has more recently suggested that the problem be "shelved as permanently debatable and insoluble."[3] Actually, the Würzburg type of phenomenology has gone out of fashion after a life-span of barely more than two decades. The history of introspectionism is thus not unlike that of the tragic hero finding himself doomed by having become true to himself.

3. The "World of Things" and Its Residue of Ambiguity

Borrowed relative stability in sensationism.—The emphasis on sensation predominant in English empiricism had lent introspection a certain measure of stability. Since sensory experiences are usually supported by the presence of an external stimulus, they are sturdy and persistent in comparison with emotions, imagery, and thought. They withstand observation better, and there is greater conformity of report from subject to subject. In this manner sensation not only shared in, but for a while carried the full load of, a slowly developing methodological ideal of inter- and intra-subjective univocality of observation. It must be remembered, however, that the cognitive security that goes with an emphasis on the boundary between organism and environment comes not so much from the subjective certainty of intra-organismic actuality as it does from the temporal continuity and the social communality of the *measured* "geographic" surroundings. The emergence, pointed out by Bentley, of the peripheral region of contact, the "human skin," as "philosophy's last

line of defense,"[4] is thus the outgrowth of a basic fallacy. So far as psychology proper is concerned, there resulted the paradox that an introspectionism sidetracked from its course by a confusion of issues with the flourishing sensory anatomy and physiology of the period could, by virtue of the very type of distorted introspection practiced by it, appear to be more of an exact science than an unadulterated phenomenological introspection ever could be capable of becoming.

Thing-language and approximate perceptual thing-constancy. —The development of the psychology of perception from strict sensationism to gestaltpsychology has found its continuation and climax in a phenomenology of the "worlds" of color, touch, etc., with their unique types of global perceptual attributes (such as apparent "surface-texture,"[6] or "solidity"). This third type of approach restores the thing-language of daily life to its rightful place in the inventory of immediately given experience.

This development has found a rough parallel in a series of shifts in experiential positivism. Mach's punctiform sensations give way, in Russell,[5] to an emphasis on organized "perspectives" and the "aspects" of the things that may be constituted from them; a further step leads to Carnap's and Neurath's "thing-language" and "language of daily life" (this *Encyclopedia*, Vol. I, No. 1, pp. 52 ff. and 19).

In the same seemingly paradoxical manner in which sensationism was said to support the ideal of objectivity, the thing-language gains a spurious foothold in the realm of *measured* physical predicates through the surprisingly invariant yet not entirely foolproof correlations of the experienced "things" with these measured properties, known in psychology as "approximate perceptual thing constancy" (see sec. 20). Since the correlations of phenomenal thing-attributes are predominantly with the more permanent measured properties of remote solid bodies —sometimes called "distal" stimulus variables—rather than with the comparatively shifty direct—or "proximal"—stimuli impinging on the sensory surfaces, the semblance of stability is even greater than for sensation. As Lenzen (this *Encyclopedia*, Vol. I, No. 5, pp. 29–30) has phrased it, the mechanism of thing-

constancy in a sense displaces the "partition between object and observer" into the distant environment. The phenomenon of perceptual thing-constancy, significant in its relation to certain notions in the mathematical theory of sets, has time and again drawn stimulus and response together in the minds of philosophers somewhat in the manner of naïve realism, and has helped to ban their separation as an "unnecessary duplication" of entities, to be eliminated in accordance with the parsimony requirement known as "Occam's razor." It is obviously for the same reason that the thing-language has sometimes been considered equivalent to the objective or "physicalistic" language. The inherent difficulties can in the end be resolved only by replacing the traditional dualism of "ontologically" contrasting realms or metaphysical substances, such as subject and object, or mind and matter, by a simple duality or distinction, within one and the same physicalistically conceived universe of discourse, between organism and environment, or between stimulus and response.

One of the most cogent reasons for the conceptual separation of stimulus and response in the psychology of perception is the fact, mentioned above, that the cognitive mechanism is far from perfect or foolproof or, more generally, that there is inter-regional ambiguity. Early gestaltpsychology had received its perhaps strongest impetus from the realization, notably by Benussi, that there is a marked "gestalt-ambiguity" in the perception of identical stimulus configurations. A picture first seen as two profiles facing each other may suddenly shift in perception to give the radically different impression of a goblet formed by the same outlines. Both the terms "profile" and "goblet" designate "thing-predicates" in the naïve-realistic language of perceptual units. Although figure-ground reversals like the one described may play a minor role when it comes to the description of physical apparatus in the laboratory, they are nonetheless significant enough in principle to upset the unique consistency required of a truly physicalistic language. No sharp line can be drawn between the ambiguities of ordinary thing-perception and those of "physiognomic" responses to human expression or even those

of aesthetic appreciation. Ambiguity is the mainstay of the so-called "projective techniques" (such as the inkblot test by Rorschach) in which gestalt-ambiguity is purposely provoked to throw light on the organization tendencies and personality of the responding observers.

It is at this point that psychology must insist on greater precision and scrutiny than even the physicist may find necessary in his routine description of experimental situations or manipulations. For the psychologist, perceptual thing-impressions or their physiological counterparts—or the ensuing overt verbal or behavioral manifestations—are definitely "responses," to be contrasted with stimuli. The distinction which Carnap[6] draws between "predicates of the thing-language" and "perception terms" can be maintained only if the former are restricted to data of measurement, which often seems tacitly implied. Unless this is done, the two sets quoted must both be treated as part of the response world. Their separation is a case of undue duplication of experience similar to the juxtaposition of "content" and "immanent object" in classical intentionalism (sec. 16). It is here where we must apply Occam's razor. Wherever description of stimuli in the "thing-language" is taken as a practical substitute or temporary expedient, there must be ultimate recourse to the strict forms of measurement in all cases of disagreement or doubt.

Historical steps in the separation of subject and object. "Copernican revolutions."—In historical perspective the present discussion appears as part of the struggle for a necessary differentiation within the originally unstructured field of naïve personal experience. The fact that this struggle took place primarily within the framework of the rationalism-empiricism controversy makes this latter philosophical issue one of the most important for psychology (for its general discussion see Santillana and Zilsel, this *Encyclopedia*, Vol. II, No. 8; see further Northrop[7])

The development of this differentiation begins with Democritus' skepticism of sensory qualities, occasioned by observations of the ambiguity of stimulus-response relationships in the field

of smell and taste. His views are developed in Locke's doctrine of "secondary qualities." That the latter's "primary qualities," such as size, shape, and motion, were likewise not free of ambiguity and deception had not been made sufficiently pivotal prior to the psychological emphasis on gestalt-ambiguity, mentioned above, and on the related subject of gestalt-"inadequacy" (or "geometrical illusions"), toward the close of the last century. As has been pointed out especially by Metzger,[69] the gestaltpsychological emphasis on the contribution of the individual to the perception of form leans heavily upon Kant in the way of general background attitude of a rationalistic nature, especially upon the doctrine of the subjectivity of space. (Concerning Kant as a rationalist see also Reichenbach.[2])

Crucial turns in the history of ideas, such as that just mentioned, are sometimes described as "Copernican revolutions." They define a succession of increasingly threatening blows to the pride of the ego; in psychoanalytic terms, the history of science is one of "retreating narcissism," or disentanglement of the objective from the subjective and wishful. Copernicus himself dethroned man's planet as the center of a faraway universe; Darwin dethroned the human species as the absolute master of the animal kingdom; Freud went still further and dethroned the conscious ego as the true representative of our own motivational dynamics. Kant and gestaltpsychology complete the picture by showing the subjectivity of the thing-language. Discovery of an ambiguous rather than univocal relationship between distant regions or variables seems to be at the root of most of such revolutions; the new "schools" protest the respective "constancy hypotheses" (see secs. 2, 16, 21).

Since gestaltpsychology is based on the recognition of a previously neglected fundamental ambiguity, it is not astonishing that it has in some quarters—quite expressly so in the school of E. R. Jaensch which dominated the German psychology of the late 1930's—met with an emotional resistance quite comparable to that opposing psychoanalysis. The importance of a more or less generalized "intolerance of ambiguity" for the cognitive and emotional outlook of personality as a whole and for social attitudes has been pointed out by Frenkel-Brunswik.[8]

It may be noted that the brief but significant history of psychology by Hulin[9]—the only book on the subject written under the aspects of a single "leitmotif"—sees the development of psychology as a gradual recognition of the differentiation between the subjective and the objective, although the overcoming of animism and anthropomorphism are stressed to the

comparative neglect of the perceptual or other developments set forth here.

That there are remnants of stimulus-response confusion even in the field of the sensory qualities proper may be exemplified by the fact that the response term 'pitch' is sometimes used as a synonym for the stimulus term 'sound frequency.' Such recent studies in "multidimensional psychophysics" as Stevens' stimulus-response analysis of tonal attributes,[23] showing an influence of sound intensity upon the pitch response, demonstrates the equivocal relation of the two vocabularies and thus renders their separation mandatory.

4. Objectivity, Methodological Physicalism, Operational vs. Experiential Positivism

Measurement as observation of point-coincidences in space-time.—We now turn to a more positive delimitation of physical measurement. It forms an important part, although only a small one, of thing-perception. Its selection is based on the challenge, inherent in the ideal of "objectivity," of finding a class of observations which, although not necessarily of the highest possible subjective convincingness, display the highest attainable degree of consistency in the sense of agreement among different observers and, for the same observer, from one observation to another. Observations satisfying this requirement of inter- and intrasubjective univocality may then be used to set up a network of knowledge maximally free of contradictions. As has been pointed out primarily by Schlick and by Eddington,[19] of all observations those of the coincidences of points in space-time come closest to fulfilling this requirement. They are, therefore, used as the protocol operations in objective, physical "measurement" (see also Lenzen, this *Encyclopedia*, Vol. I, No. 5, p. 90); the term 'introspection' in the technical sense has come to refer to all the vast remainder of givennesses, including most of thing-perception.

A further advantage of drawing the line between the subjective and the objective in the manner described is given by the fact that, even from a purely introspectionistic point of view, point coincidences can in all likelihood much more easily be demarcated from the rest of the "experiences" or "givennesses" than sensations or any of the other types of cognitive content can be from one another. The physicist's universe, and with it

objective psychology, thus emerges as an offshoot of a particularly salient kind of experience.

"Observational reliability" as a statistical measure of objectivity.—In the sense of section 3, one may in general expect a gradual decrease of stability and agreement of response as one proceeds from the observation of point-coincidences to thing-perceptions, images, thoughts, valuations, and subjective feelings or moods. This may be used to lend an element of continuity to the distinction between introspection and objective observation. The traditional dichotomy between the subjective and the objective could thus be moved, as Comte would say, from the metaphysical to the scientific level or, as Lewin[29] would say, from the Aristotelian to the Galileian mode of approach.

We may relate the concept of objectivity to what is known to the psychological statistician as test reliability. In the establishment of the reliability of a psychological test there is repeated application of the test to a sample of individuals; in replacing "test" by "type of observation," and in applying this observation to a sample of environmental situations or situational elements, we may define objectivity as observational reliability.[30] Either only one individual (subject) is involved, repeating the observations at least once (intra-individual observational reliability), or each of at least two individuals has to do one observation per situational element (intersubjective observational reliability). "Objective," then, is a class of responses yielding maximum reliability coefficients within or between individuals facing a common geographic situation or situational element.

In taking a strictly empirical point of view, one may be satisfied to ascertain by statistical procedure that point-coincidences happen to be unsurpassed in this respect, i.e., that pointer-reading types of observation are the most reliable methods in testing the surrounding world. It will be only such comparative oddities as, say, hallucinations which will spoil the ideal correlation, underscoring the inescapable relativity of the concept of objectivity, and thus the need to express its degree quantitatively. An alternative, deductive defense of point-coincidence as an epistemologically

unique kind of givenness may take its start from the observation, made below, that it is the only case of observational equality in *all* respects.

Coordinate language in communication and theory construction.—The requirement of inter- and intrasubjective univocality extends from observation to communication within and between individuals, as well as to the construction of hypotheses, theories, inferences, and predictions. This is fulfilled by the use of a "coordinate language" which permits us to locate observations and inferences in a system of mathematical coordinates in terms of topological or metric relationships, avoiding reference to "qualitative" terms.

Basic observational protocols dealing with point-coincidences use the relational terms 'equal' and 'unequal.' While in ordinary subjective equivalences as studied in psychophysics the surroundings or other characteristics are in general different for the two items to be compared, a point coincidence is an equivalence in *all* respects at the same time; this reduces to a minimum the sources of disturbance and renders unnecessary the "naming" of the judgment in terms of any sensory or other introspective "qualities" which subjectively designate the "attitude" taken in the process (see secs. 3 and 20). The greater persistency of impressions of inequality in the face of simultaneous impressions of equality has induced Dubislav[13] to consider "immediate judgments of simultaneous inequality"—rather than those of equality—to be "the most certain facts of perceptual observation." Stevens' emphasis on "discrimination" as the operational successor of sensory qualities[23] reflects a similar point of view.

Cooperational rigor vs. subjective clarity.—The shift of emphasis, in the search for certainty and truth, from intuitive clarity and subjective self-evidence as stressed by such rationalists or neo-Scholastics as Descartes (sec. 1) or Brentano (sec. 17), to rigor in the sense of inter- and intrasubjective consistency, univocality, and coherence as stressed by the empirical sciences, implies a fundamental expansion of scope from the one to the many, from the self-contained to the relational, the introvert to the extravert, the private and encapsulated to the public and socialized. Experiential units no longer stand in isolation but are viewed in connection with one another. The important question becomes that of stability within and among observers; there develops a certain self-imposed, democratic restraint with

respect to all those types of givennesses which are found to lead to disagreement, regardless of their impact in consciousness. Ness's[11] term 'cooperational univocality' points to the social coordination of knowledge thus achieved. Or, to use a term by Morris,[12] any sign used in science should be a "comsign";[12] it should have the same signification to the organism which produces it that it has to other organisms stimulated by it.

In further support of the suggested shift of emphasis from intuitive clarity to cooperational rigor it may be mentioned that some relatively unnoticed studies within the Würzburg school (sec. 2) have exposed, by means of objective methods of checking, the frequent fallaciousness of introspections regarding object-reference, certainty, and clarity.[48]

Methodological positivism and operationism.—Since criteria of objectivity were first developed in physics, the striving for objective standards may also be designated as "methodological physicalism" (see Carnap and Neurath, this *Encyclopedia*, Vol. I, No. 1). Concepts become scientific when they are anchored in physical observation, directly or by mathematized construction procedures. Anchoring may be either by definitive "definitions" after logical analysis or by tentative "reduction" sentences which will allow to put all statements concerned to the test of verification or, less stringently, to that of confirmation. The view that scientific statements have meaning only by virtue of the concrete operations that enter into the definitions of the concepts employed is known as "operationism."[1] Care must be taken that the concept of "operation" be sufficiently specified in the sense of methodological physicalism lest relatively casual testing procedures including introspection be placed on an equal footing with objective methods. Furthermore, equivalence of certain operations which are different from a purely manipulatory point of view—such as direct measurement vs. triangulation or other inferential ascertainment of length—must be properly stressed (see sec. 6). Equivalence of operations may have to be established by empirical correlation of results rather than on deductive grounds.

Methodological physicalism and operationism are part of a general "positivistic" tradition. Their psychological counterpart

is the school of behaviorism (secs. 18 and 20) which attempts to base all its statements on the movement or physiology of organisms taken as physical bodies. Methodological or "operational" positivism thus must be distinguished from the epistemological, "experiential" positivism of the Mach tradition in which the immediately given of introspection is uncritically identified with sensation (see sec. 1). A distinction must further be made between methodological and thematic physicalism, the latter being given by the uncritical emulation of the real or alleged particular aims and problem content of physics by other disciplines (see sec. 11).

5. The Futile Search for "Criteria of Consciousness." Verbalization

Docility and other functional criteria.—As was pointed out by Bekhterev[70] and by others, among them Boring,[50] there is no objective indication of the presence of consciousness that could withstand scrutiny, and thus little possibility of successfully "reducing" consciousness to physical observation. This fact must be stressed in the face of numerous historical attempts to link "mind"—primarily in infants or animals—to physical criteria.

Some of these suggested criteria are "morphological," such as the presence of a nervous system or of neural specialization (Yerkes). Others are "functional," such as "discrimination" (selective response) or "choice" (Bergson), modifiability of reaction through individual experience in learning—or "associative memory"—also called "docility"—(Romanes, Lloyd Morgan, Loeb), and variability of reaction—sometimes called "initiative"—(Yerkes, Margaret Washburn). Other authors have suggested such behavioral criteria as use of tools, specified response to facial expression, use of representational language or other symbols, ability to play or to respond to fictitious situations, or apparent purposiveness as given by reaction to an end, especially if the situation or the response is novel.

In surveying some of these suggestions, Washburn[14] comes to the conclusion that they all elude precise definition. Some of them, such as learning or docility, can readily be traced to unicellular organisms or even to the simplest inanimate processes. She further points out that Yerkes has rendered the problem a matter of degree by emphasizing the "rapidity" of learn-

ing as a criterion. Since she admits neural specialization as a second, likewise quantitative, criterion, the problem of "evidence of mind" is made to appear at best as a matter of multiple-criterion and probabilistic, rather than of one-criterion and absolute type of reduction. Carr[14] deplores our "lack of knowledge of any decisive and unambiguous criterion of ideas in man" which in itself precludes any satisfactory approach to the problem as applied to animals.

Introspectionistic and behavioristic use of verbalization.— Special attention should at this point be given to an issue all too often left in confusion, that is, the relationship of verbal report and introspection. Behaviorists have often felt that there was an unholy alliance, if not an intrinsic association, of verbal report and introspection. Watson somewhat naïvely decreed 'unverbalized' to be the behaviorist's translation of 'unconscious' and somewhat arbitrarily identified thought with "laryngeal habits"; there was a marked decline in the use of verbal report in favor of "overt behavior" during the heydays of classical behaviorism (see sec. 18).

It should be noted that the presence or absence of overt verbalization plays a relatively minor role in the historical list of suggested criteria of mind given at the beginning of this section. Introspectionism is possible without the use of verbal report, such as in all attempts at interpreting facial or pantomimic expression in an intuitive process of empathy or in an explicit inferential search into another person's mind by analogy with one's self. On the other hand, it is possible to utilize verbal report without falling back upon introspectionism. This is the case whenever words are not taken in their common-sense or dictionary meaning but anchored, by statistical correlation, in overt behavior or its results.

For example, placing a check mark in the space for "No" opposite the statement "I would like to be an actor"—a verbal response whose similarity to point-coincidence is obvious—has been found to have the pragmatic "meaning" that the subject has potentialities to become a successful engineer.[15]

Such semantic analyses as the anchoring of the traditionally "philo-

sophical" concept of "truth" in the language system of daily use, attempted by Ness,[16] also belong in this context.

To be distinguished from the overt behavioral is the dynamic, or motivational, anchoring of verbal report which likewise often discards common-sense meaning in favor of objective methods of ascertainment (see sec. 16).

II. The Functional Unit of Behavior and the Level of Complexity of Psychological Research

While differing in important points of elaboration of their systems, behaviorists have been rather unanimous in their definitions of the basic structural characteristics of behavior. These definitions contain all the necessary conceptual elements for making explicit the general characteristics of an approach necessary to establish objective psychology on an adequate level of complexity. But they have left to the general methodologist the actual drawing of conclusions concerning the design of a type of research that would match the pattern of behavior itself.

6. Stabilized Achievement and Vicarious Mediation. The Lens Model

Definition of the subject matter of psychology seemed obvious, or else a matter to be left to the philosopher, so long as the fundamental conception of psychology was introspectionistic. Thus the chief exponents of early experimental psychology, Wundt and Titchener, acted in conformity with philosophers of the period when they declared, respectively, that psychology deals with "immediate"—as contrasted to "mediate"—experience, or with experience as "depending on"—as contrasted to "independent of"—an experiencing person.

Vicarious functioning and focusing on "ends." Purposive behavior.—All this changed with the shift toward a behavioristic conception of psychology at the beginning of the present century. The subject matter of psychology now was physical occurrences; but psychology could obviously not cover all the physical occurrences, not even all physical or physiological occur-

rences within or around organisms. Was the incidental stumbling of a person over an obstacle to be considered behavior or nonbehavior? The theorist of classical behaviorism, Albert P. Weiss,[17] provided the crucial model to deal with doubtful cases of this kind in his "raindrop analogy" of purposive behavior. He pointed at the convergence of originally diverse occurrences toward a common characteristic end stage in behavior and compared it with the way raindrops originally scattered over a wide area are eventually carried to a common point in the sea.

An effort had thus been made to link the most prominent descriptive feature of behavior, characterized as "striving toward a goal" or as "equifinality," to simple physical processes. The latter were called upon originally to demonstrate the possibility of a mechanistic reduction or "explanation." But by the same token the existence of the pattern itself, as a fundamental descriptive characteristic of behavior, was acknowledged.

Two features are involved in this pattern. One is what may be called "stabilization" of the end stage, to be labeled "terminal focus" henceforth. The second is the diversity of preceding stages. Another prominent representative of classical behaviorism, Hunter,[18] concentrates on the implications of this second feature upon the flexibility and exchangeability of pathways relative to an end when he elevates "vicarious functioning" to the role of the defining criterion of the subject matter of psychology. The same holds for certain ideas by Holt, while Hobhouse has conceived of the possibility of remote focusing.[19] In the case of Meyer's "psychology of the other-one"[19] the emphasis is not so much on vicariousness proper than it is on the related aspect of the "concerted" character of behavior as exemplified by the cooperation of several limbs in action; this is comparable to the "multiplicity" of mediation which is important in perception (see below). In listing the essential postulates for a human "robot," Boring[35] notes, under "vicarious response," that "there is little that will make our robot seem more human than this ability to choose one means after another until the goal is reached."

All concepts and considerations of this kind stress the equiv-

alence and mutual intersubstitutability of certain activities, habits, sense departments, or bodily organs for one another in behavior. They form "hierarchies" either in the sense that some of the alternatives may be more useful than others (e.g., by possessing a greater probability of resulting in the characteristic end stage) and/or in the sense that some alternatives are better established in the organism than others (e.g., by more effective learning). It is primarily the latter feature which Hull had in mind when he coined the phrase 'habit-family-hierarchy'[50] to describe and to explain patterns of vicarious functioning in overt behavior.

The terminal reference which is always at least a tacit constituent of such models is made part of the label itself when Tolman[54] operationally redefines the "purposiveness" inherent in all behavior as "persistence through trial-and-error, and docility, relative to some end." There is a variety of "means" to each end, and this variety is changing, both variety and change being forms of vicarious functioning. Rats are found to persist in this manner until arrival at the goal and its consummation is followed by quiescence. And the stumbling of a person over an obstacle is, according to this definition, to be considered behavior if, and only if, it can be shown to be linked with a group of events (or habits) running off in series until a certain result is brought about, say, damage to the individual ("accident-proneness," which may be part of a broader "suicidal" purpose).

Backward extrapolation in perception. Variability and multiplicity of mediation.—In overt behavior an additional initial focus is frequently given by a hunger stimulus or by a "central" motivational state. The initial focus forms a counterpart to the external terminal focal state of arriving at the food. The earliest behaviorists have usually seen the main problem in the convergence of causal chains toward the terminal focus. However, in the field of cognitive processes, the initial focus is not an event within the organism as is motivation; rather, it is an external variable. In the case of perceptual thing-constancy, the measured sizes of physical objects in space—constituting a "distal" stimulus variable (see sec. 3)—turn out to be, in a figurative

sense, backward extrapolated by the terminal perceptual size response irrespective of their distance from the observer. The latter feature implies a variability of "proximal" retinal projections; together with "cues" or "criteria" for a further distal variable, depth, in a process of "multiple" mediation,[77] these projections form patterns of vicarious functioning in establishing the final size response. External initial foci of this kind possess a status symmetrical to that of the external terminal foci encountered by the student of overt behavior; in both cases a mechanism of vicarious mediation is needed to correlate external to internal events with a sufficient degree of likelihood.

Since there is no perceptual cue which would be available under all circumstances or is completely trustworthy (see below), the perceptual system of higher organisms must for most types of perceptual attainment develop what the present writer has suggested calling an "or-collective" or an "or-assemblage" (*Oder-Verbindung*) of mutually interchangeable cues vicariously mediating distance or other situational circumstances to the organism (see sec. 20).[77] Since cues form a hierarchy just as do means, we may also speak of a "cue-family-hierarchy," in conformity with Hull's "habit-family-hierarchy." It is characteristic of the formal convergence of lines of psychological research differing in content (see sec. 19) that the two concepts were introduced independently in the same year.

The lens analogy of the unit of achievement.—A stabilized or relatively stabilized connection between focal variables taken as classes of events rather than as individual occurrences, as established through vicarious functioning either in perception or in overt behavior, may be characterized as an "accomplishment" or "achievement" (*Leistung*)[77] (for further specification see sec. 7; for discussion see also Carnap, this *Encyclopedia*, Vol. I, No. 1, p. 48). The term 'function' will likewise in the main be used to characterize wide-arched dependencies of this kind, in contradistinction to the more microscopic aspects of the "functioning" of physiological processes. Focal variables outside the organism may be called "functionally attained" (*intentional erreicht*). From a "molar" point of view, comprehensive patterns of this kind may be taken as the dynamically integrated

and effective units of behavior. Organisms may in this sense be characterized as "stabilizers" of events or of relationships.

The total pattern involved, when viewed as a composite picture of numerous cases of individual mediation from initial to terminal focus, bears resemblance to a bundle of rays scattering from a light-source and brought back to convergence in a distant second point by a convex lens. A generalized "lens model" for stabilized functional units is shown in Figure 1.[20] While correlation between the focal variables is assumed to be relatively high although in general not perfect (see sec. 7), those of each
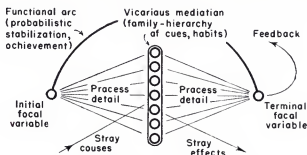

Fig. I. The lens model: Composite picture of the functional unit of behavior

focus with the single elements or chains of mediation may be low. A semicircular arrow is appended in the figure to the terminal focus to indicate that lens patterns do not stand in isolation but are apt to reflect back upon the organism in a future state in what is now sometimes called a "feedback" loop (see sec. 23), such as when arriving at the food is followed by satiation and reinforcement of the preceding behavior ("law of effect"; see sec. 18). Further lens patterns may be involved in this process.

Considering the relative chaos in the regions intervening between focal variables, focal connections may also be called "interrupted." Although they do not require action-at-distance, they are relations-at-distance. Further formal characterization of such connections may be given in terms of the mathematical classification of relations by Russell.[21] Under this aspect they appear as asymmetrical (directed) and as transitive (carrying

over from one focal pattern to the next).[77] This analysis may also be used for an operational definition of "meaning."

The broadest context into which behavioral units may ultimately be fitted is "(probable) survival" as a descriptive fact of life. The concept of survival, established by Darwin, Tyler, and other biological and sociological functionalists of the nineteenth century, is extensively used by Hull in his recent formalization of classical behaviorist theory (sec. 22). For examples of artificial stabilization mechanisms see section 23. It may be added that even lesion in the literal sense of the word are, when taken as a class and as imbedded within a stabilization mechanism, to be found only within, or as products of, higher organisms.

The principle of vicarious functioning has also been used in semiotic by Morris,[12] in his concept of "sign-family." The necessity of establishing, in the "research behavior" of psychologists, equivalence among various "member-operations" with reference to the final term to be defined, as connected with the problem of avoiding unmanageable multiplicity of concepts in "operationism" (see sec. 4), has been discussed by Ness,[11] by Stevens,[1] and by Israel.[1]

Central-distal vs. peripheral focusing of achievement.—Recent psychology has shown that variables located in certain "areas," "layers," or "regions" of the environment or of the organism seem more often to be focal than others. Some of the most crucial changes of emphasis in contemporary psychology are based on the recognition of the relatively nonfocal, vicarious, "generalized" role of the sensory as well as of the motor periphery, coupled with the comparatively focal character of the central as well as the distal regions, both situational and historical, in the case of higher animals at least. This recognition defines the shifts from classical to molar behaviorism, from sensationism through gestaltpsychology to the perceptual thing-constancies, while the inhomogeneity of the developmental time continuum—with a focal area in early childhood not anticipated in the academic psychology of learning—is a discovery of psychoanalysis (see Chaps. I, IV, and V; a survey of areas is given with Figs. 2–7).

7. Organismic Adjustment as a Probability Function

Limited "ecological validity" of cues and means. Partial vs. total cause-and-effect relationships.—As was pointed out by the

writer in greater detail elsewhere,[77, 1] any organism has to cope with an environment full of uncertainties. Forced to react quickly or within reasonable limits of time, it must respond before direct contact with the relevant remote conditions in the environment, such as foodstuffs or traps, friends or enemies, can be established. The probability character of intra-environmental relationships, their limited "ecological validity,"[30] becomes of concern in two regional contexts: on the reception or stimulus side as the equivocality of relationships between distal physical or social objects and proximal sensory stimuli or cues, and on the effection or reaction side as the equivocality of relationships between proximal outgoing behavioral responses, or means, and their more remote distal results and effects.[78] In one or the other of these ways behavioral responses are, to apply a saying by Thurstone, of necessity based upon "insufficient evidence."

Another way of stating this state of affairs is by pointing toward the fact that the relationship between the mediated event and its mediators is one of probable partial causes and partial effects (see also Reichenbach[22]). In the case of the depth-cue of geometrical "perspective," a trapezium-shaped image on the retina may have any one of several possible lines of causal ancestry. It may be historically caused either by a rectangle actually tilted into the third dimension—the valid instance of the cue—or by an actual trapezium in frontal position —an invalid, misleading instance of the cue. This is due to the fact that neither an actual rectangle nor an actual trapezium would constitute the total cause of the proximal image, among the remaining conditions being the rotation of the object in space.

This picture must be set up to counterbalance the notion of univocality based on the tacit limitation of causal considerations to relationships between total cause and total effect as frequently taken for granted by the traditional logician or experimental physicist with his more fully developed, explicit access to the relevant elements of a situation and his rational means of analyzing them. Such relationships have been de-

scribed by the "strict" laws of the classical natural sciences and have been idealized in such universal conceptions as Laplace's world formula.

Impossibility of foolproof distal achievement. "Functional validity." Quasi-rationality.—In line with the inherent probability character of object-cue and of means-end relationships, gross organismic coming-to-terms with the environment can thus never become foolproof, especially so far as the more vital remote distal variables are concerned. It is in this sense that, as William James has phrased it, perception is "of probable things." In the terminology of Reichenbach's probabilistic empiricism, behavior and the inferences implicit in it must retain a certain "wager-" or "posit"-character.[22] Perceptual and behavioral functioning is spoiled much in the manner in which stray rays (Fig. 1) are apt to interfere with perfect focusing. Imperfections of achievement may in part be ascribable to the "lens" itself, that is, to the organism as an imperfect machine. More essentially, however, they arise by virtue of the intrinsic undependability of the intra-environmental object-cue and means-end relationships that must be utilized by the organism; these are comparable uncontrolled lateral light-sources.

It is for this reason that the concept of achievement or of cognitive "correctness" must be defined in psychology in the generic terms of over-all statistical correlation between variables as classes rather than in terms of single hits or misses of judgment or of action. "Achievement," in the sense of the probability for an initial focal event (say, a measured stimulus) to be followed by its terminal counterpart (say, the correct perceptual estimate), may, then, be defined as "functional validity" and measured by a correlation coefficient. Here it is the environment which is being tested by the reacting organism, rather than vice versa as in "test validity" (compare with "observational reliability" *versus* "test reliability" as developed in sec. 4; for further details see Brunswik[30]).

All imperfections notwithstanding, the use of cues and the taking into account of several variables at the same time injects an element of reasoning into stabilization mechanisms.

Implicit reasoning of this kind may readily be made explicit by modern "cybernetics," or by "mathematical biophysics" (see secs. 22 and 23). In the field of the perceptual constancies, it has first been recognized in Helmholtz' doctrine of "unconscious inference" (*unbewusster Schluss*).

The present writer's emphasis on the cognitive achievement of the perceptual constancies, brought about by the utilization of cues, is, as Boring[23] has phrased it, a "modern equivalent" of the doctrine of unconscious inference. The reason why it is not merely a revival of this doctrine may be expanded as follows. It sheds the introspectionistic entanglements of "unconscious inference," such as of Helmholtz' untenable assumption that we are dealing with a mechanized form of originally conscious processes. Nor does it accept the notion that the perceptual constancies function exactly like rational inferences save for the fact that they are not conscious. Rather, the crucial change is a functional-objective one, based on the experimental demonstration of perception as a subsystem of the total personality that is relatively autonomous of reasoning proper[77] and distinguishable from the latter in terms of both achievement and mediation. Cues of perception proper are found to be sluggishly established as probabilistic stereotypes; once established, they act with a quick efficiency full of peculiar pitfalls. More recently, an attempt was made directly to compare perception and thinking in terms of differences in the statistical distribution of error.[30] All evidence may best be summarized by designating perception as a "quasi-rational" rather than a rational system. Perception is what Werner[24] has labeled an "analogous function (or process)" to reasoning, more primitive in its organization but vested with the same purpose (in the behavioristic sense of this term).

In an attempt at rational reconstruction of the ways of the quasi-rational, with its reliance on vicarious cues each of which is of limited validity, one may best refer to a remark of Thorndike comparing the impressionistic or intuitive judge of men to a device capable of performing what is known to statisticians as multiple correlation. This is a device, related to what cyberneticists have called redundant communication (sec. 23), by which the probability of individual correctness may be increased but not perfected to the point of certainty. By contrast, man-made gun or tank stabilizers and the related "thinking machines" may, within the limits of their technical purpose, perfom in a practically foolproof manner. This is due to the fact that they can usually be built with a concentration on a few cues of maximal trustworthiness and thus dispense with the services of cues of limited validity. In the case of distance or rotation from a frontal plane, to quote just one example, triangulation may be used in prefer-

ence to the untrustworthy cues so often employed by perception proper, especially in monocular vision.

8. The Molar Approach: Focal and Macro-mediational Reference

The methodological postulate of behavior-research isomorphism.—The suggested definition of the functional unit of behavior in terms of the lens model is a case of "persuasive definition" in the sense of Stevenson.[25] It is well founded by the fact that—so far as is known to the present writer—no one attempting an objective definition of behavior in terms of a structured pattern was able to think of anything but vicariously mediated periodic equifinality (sec. 6). The fruitfulness of the definition is demonstrated by the fact, to be pointed out in Chapter V, that most contemporary movements in psychology explicitly or implicitly use this scheme or at least some of its basic features.

In applying the lens model to psychological methodology, one may metaphorically paraphrase Spinoza's parallelistic credo and demand that the "order," or pattern, of research "ideas," or design, should be the same as the pattern of the "things" studied, which in our case is behavior. Research may be said to have reached an adequate, "functional," or "molar" level of complexity only if it parallels, and is thus capable of representing, behavior in all its essential features. We may call this the methodological postulate of behavior-research isomorphism.

Focal alertness.—In particular there should, first, be no restrictions upon the scope of the search for focal relationships. Research should focus wherever behavior focuses or may focus. Since foci are defined by intercorrelation with other foci, a great variety of inter-variable and inter-area relationships must be studied, including some that arch out very far in space or time. One may adopt Boring's admonition to "tap" the organism at the right places and augment it by pointing out that the environment—both spatial and temporal—must also be tapped at the right places. In this process, central-distal emphasis seems to emerge as a particularly promising pattern of regional refer-

ence (see sec. 6 and Chap. V). The final breaking-away from traditional peripheralism was preceded by chains of painful disappointments in experimental research[3] which can be traced to a naïve oversight of the nonfocality of certain variables. Such nonfocality could have been readily anticipated if psychologists had been more effectively imbued with the principle of vicarious functioning.

Since possible reference variables and the possible intercombinations between them reach an unmanageable total, "blind" gathering of fact must seem increasingly hopeless in psychology. Like a prospector digging for oil, psychologists become increasingly reluctant to treat the field to be explored as homogeneous or nondescript. Tentative generalizations, hunches, and expectations regarding possible singularities of behavior furnish a promising basis for an imaginative theorizing-before-the-fact as to the possible "fruitfulness" of an approach.

Passive control in the macro-mediational study of vicarious functioning.—While focal correlation furnishes an over-all frame of reference, the gross degree of flexibility of vicarious functioning, that is, the range of mutually intersubstitutable processes in a family hierarchy of cognitive cues or of overt behavioral habits (means) mediating between two focal variables remains a second integral aspect of psychological inquiry. In terms of the lens model, this range is comparable to the aperture of the pupil of the eye. Tolman[74] has recently spoken of the "width" of the "cognitive map." This range or width characterizes the degree of "generality," and thus in turn the degree of functional efficiency and foolproofness of a behavioral unit. Its study "from above," that is, under the aspect of the superordinate behavioral units, may be characterized as macro-mediational.

In controlling vicarious mediation, care must be exercised not to interfere with naturally established mediation patterns. These aspects of mediation must therefore be controlled "passively," that is, be studied in a permissive laissez faire manner with respect to their free dynamic flow; there must be deliberate neglect of "active" control at least up to a certain point, despite the fact that the conditions involved either are defi-

nitely known to be relevant or are at least potential mediators bridging the gap from one focus to another. In particular, mediation must not be "channeled"[30] by allowing, say, only one of the many perceptual distance cues to function, or by providing only one path to the goal, as was the case in earlier phases of experimental psychology. Channeling of mediation leaves no room for vicarious functioning; in consequence the entire relief of focal versus nonfocal variables or regions is obscured.

As the specific merits of a bombsight or a gun stabilizer can only be fully appreciated when the conditions to be "eliminated" by these mechanisms are kept variable rather than constant, organismic achievement can unfold in its full scope and specific focalization only when there is room for expansion of the vicarious mechanisms which funnel the divergent causal chains back to convergence in a terminal focus. Any machine to be tested must be exposed to the full challenge of conditions for which it is designed, and so must any organism.

Impairment of vicarious functioning implies the breakdown of higher functions. Mental disturbance or retardation in natural development has often been described in terms of "rigidity," "fixation," "concretism," etc. An example is Krech's finding that rats with brain lesions tend to move along more stereotyped paths to the goal than do normal rats, and even will try to avoid the challenge of choice points altogether whenever possible.[26] (See also Frenkel-Brunswik.[8])

Micro-mediationism and the atomistic approach to the functional arc.—Neglect of any of the methodological requirements listed in this chapter constitutes psychological atomism of some sort. In the traditional micro-mediational tracing of sensory-neural transmission (see secs. 10 and 15) one may easily lose sight of focal relationships. On the other hand, concentration on relationships between pairs of variables far removed from each other in space or time, to the exclusion of all mediational considerations, as sometimes found in mental testing or in the purely correlational treatment of the inheritance of traits (see sec. 20), is atomistic in the opposite sense; it stresses the focal arc at the expense of checking on the scope and intricacy of vicarious functioning through which (relative) stabilization is alone possible.

9. The Molar Approach: Probability Laws and Representative Experimental Design

Statistical approach in functional and differential psychology.—Since behavior as an adaptive function is inherently bound to have its imperfections and occasional failures, the traditional nomothetic search for strict laws becomes an insoluble task when applied to it (sec. 7). The degree of univocality of results cannot exceed the degree of univocality of behavioral achievements themselves, so long as the latter are studied as class-relationships between focal variables connected with each other in the manner of part-causes and part-effects. All we can hope for with respect to molar behavior is what Reichenbach and others have called "probability laws."[22] Among these are correlation coefficients and other statistical measures of concomitant variation (see also Nagel, this *Encyclopedia*, Vol. I, No. 6, p. 25). It must be stressed once more that the probabilistic character of behavioral laws is not primarily due to limitations in the researcher and his means of approach but rather to imperfections inherent in the potentialities of adjustment on the part of the behaving organism living in a semi-chaotic environmental medium. In this sense even an omniscient infinite intellect, when turning psychologist, would have to adopt a probabilistic approach (see further the discussion with Hull referred to in sec. 11).

As was pointed out for physics by Mises,[27] only differential equations which encapsulate within infinitesimally small space-time segments can be conceived of as laws in the strict, absolute sense. Their integration into the customary macroscopic form implies incorporation of further variables which inject a probabilistic element into the law. There is an inherent tie between nomothetic approach and confinement of scope which goes beyond the routine type of analytic effort involved in any finding of regularities. (For discussion of seemingly opposite further aspects of the difference between micro- vs. macro-approach see below in this section, and Philipp Frank.[28])

Under the pressure of conditions which are in important re-

spects similar to those just referred to, correlation statistics and representative sampling as a deliberate lump treatment of inter-variable relationships was first developed in a field close to psychology, anthropometrics, soon to spread to the study of "individual differences" in psychology proper (see sec. 19). Imperfect correlation between variables, such as between the intelligence of parents and offspring, was from the beginning recognized as due to partial causes other than those included in the operational data.

The neglect of these remaining conditions, in spite of the fact that they obviously contributed to the results (see sec. 8), was, and still is, looked upon by most psychologists as a blemish on scientific exactitude, to be ameliorated as soon as practicable. In this vein, correlation statistics appeared as a mere temporary expedient eventually to be replaced by full observation of all relevant factors, with an absorption into a strictly nomothetic psychology as the final goal. In contrast to this, we must propose the statistical approach as an ultimate norm for psychology as a whole,[29] rather than as a somewhat embarrassing substitute in the relatively specific field of differential psychology. Such a reorientation is established in its more positive aspects by expanding the field of application of statistics to include the functional study of achievement through a correlation of stimulus variables with response variables as outlined in section 7. This shift defines what may be called a "probabilistic functionalism"[30] in the study of psychological laws (see sec. 20).

Situational generality in the representative design of experiments.—The call for unrestricted vicarious functioning in studying gross behavioral adjustment, voiced in section 8, injects a new element into the discussion of these issues. It is the requirement of normalcy, naturalness, "closeness to life" (*Lebensnähe*), or, with a more methodological slant, that of "situational representativeness."[30] According to the much-stressed requirement of "representative sampling" in differential psychology, individuals must be randomly drawn from a well-defined population; in the same manner, the study of functional organism-environment relationships would seem to require that not only

mediation but especially also focal events and other situational circumstances should be made to represent, by sampling or related devices, the general or specific conditions under which the organism studied has to function. This leads to what the writer has suggested to call the "representative design of experiments."[30] The correlation coefficient can then be employed as a measure of functional stimulus-response relationships as it has been accepted in the past as a measure of differential intertrait relationship. Such a generalized statistical conception of psychology would serve to establish a unity of outlook and research design hitherto lacking within this discipline.

Any generalized statement of relationship requires specification of a "reference class" or "universe" from which the material is drawn. In case of strict "laws" as aspired to in the natural sciences reference is supposedly unlimited; thus we are apt to forget that correlations have meaning only when specified as to applicability. Certain analogies existing between the interpretation of representatively designed psychological research and the problems of the generality of results connected with relativity physics have been pointed out by Hammond.[32]

By its extension of the principle of sampling from individuals to situations, representative design countermands a number of preconceived methodological notions of nomothetic experimentation, among them the "rule of one variable," i.e., the studying of one factor at a time. It further countermands the more general inclination to design experimental research in accordance with formalistic-"systematic" patterns which are too narrow to bring out the essentials of behavioral functioning although they may involve a shift to "multivariate" or to Fisher's "factorial" design.[81].

As a "methodological demonstration" of a deliberate switching from systematic experiment to a broad representativeness of stimulus conditions, the writer has undertaken a study of perceptual size-constancy in the manner of a representatively designed experiment in social perception.[30] Correlations of .95 and over—high for differential psychology but probably not uncommon for functional stabilization mechanisms—were obtained between measured distal object sizes and their perceptual estimates in a representative sample of daily-life situations involving a wide variety of sizes and distances; correlations of the estimates with proximal (retinal) image sizes were low by comparison.

Macro-statistical approach in psychology vs. micro-statistical approach in physics.—Much has been written about the recent shift in physics from "strict" laws—also called "causal" or "deterministic" laws—to a statistical, probabilistic point of view. In a general way such a shift lends support to probabilistic claims or programs in other sciences. Yet there are important differences, and the analogy must not be pressed, especially in view of what has been said above in this section concerning the association between the statistical and the molar rather than the molecular approach in psychology.

In thermodynamics as well as in quantum physics—the two major points of inroad of the statistical approach in physics—it is the microscopic features that present themselves either as intrinsically not fully predictable or at least as in need of statistical lump treatment for practical reasons (see Philipp Frank, this *Encyclopedia*, Vol. I, No. 7). Univocality is for most practical purposes restored when we proceed from the microscopic to the macroscopic level, through the involvement of extremely large numbers of particles.

Psychological correlations, both of the differential and of the functional-stabilization kind, on the other hand, are erratic primarily when viewed macroscopically. It must also be noted that the main difficulty lies not in multiple dependency of reactions on a series of factors—a fact relatively easily coped with by the nomothetic approach—but in the impossibility or theoretical-methodological inadvisability (or both) of exerting (active or passive) control over the relevant, contributing factors. Bohr[33] concurs that the situation in physics is "essentially different from the recourse to statistical methods in the practical dealing with complicated systems" (to which latter alternative the situation in psychology essentially belongs).

We may illustrate the difference by an example. In studying the differential psychological problem of the inheritance of personal traits it is found that the "intelligence quotients" of fathers and sons correlate about .4. Two features must be emphasized here. First, in order to ascertain this result, one must test *individual* fathers and sons, and, what is more, the computation of the correlation coefficient utilizes the results individually. And, second, while the coefficient itself may be taken as an over-all evalu-

ation of one among several contributing factors operating with a certain consistency if large numbers of individuals are investigated, the important psychological task of predicting *individual* I.Q.'s retains some intrinsic hazards. In thermodynamics (in contradistinction to the kinetic gas theory designed to "explain" its laws) the individual molecule maintains its identity neither in observation nor in prediction, and the relation is not expressed by a correlation coefficient. To duplicate this situation in our example from psychology, one would have to devise measures of intelligence for the pooled resources of large populations of fathers and of sons which could not be separated into their component individuals, and hence the problem of individual prediction could not even be raised.

It may be added in this context that the physicist Bopp[33] has recently suggested an extension of correlation statistics which would be suitable for the description of quantum processes. Both classical correlation and quantum physical correlation constitute special cases of a more general correlation theory.

Breaking the gross functional units of behavior up into fragments is apt to increase precision. Thus we have relatively satisfactory laws of simple color vision or of the functioning of a single depth cue (especially when the cue is easily identifiable physiologically, as is, e.g., binocular disparity), while the more complex and vitally more relevant functional stabilization mechanisms of perceptual size- or color-constancy are, in spite of their impressive achievement, nonetheless found to be far from foolproof. The historical neglect of the complex in favor of the simpler problems constitutes but one of the cases of escape from the behaviorally macroscopic, or "molar," into the behaviorally microscopic or "molecular" level. In fact, some of the most outstanding formalizers of psychological theory of the present, Hull, Rashevsky, and to some extent even Lewin, have paid the price of becoming relatively molecular by a confinement of their scope to the organism as contrasted to its environment, or even to the central layer, sometimes with a quasi-physiological slant, in return for undeniable progress in the formalization of psychological laws (see sec. 22).

Briefly we may say that, while physics tends to be microstatistical and macro-nomothetic, psychology tends to be macrostatistical and micro-nomothetic. The fact that physics has better chances to find strict laws when becoming more macro-

scopic while psychology has better chances when becoming more microscopic is not a paradox; however; micro-psychology deals with phenomena of an approximately similar order of magnitude or coarseness as those of macro-physics, while both above and below this stratum there is less stringent orderliness. Yet, by comparison within its own size order, purposive behavior forms a region of outstanding texture. The existence of outstanding regions of focusing and of regions of comparative vicariousness in the organization of behavior itself as discussed above (sec. 6) is another type of the inhomogeneous distribution of orderliness in the world. In the light of historical perspective, it is psychology that seems to be called upon to deal with the relatively (although far from absolutely) orderly types of highly macroscopic life-occurrences as schematically represented by the lens model.

10. Descriptive and Reductive Theories. Law, Inference, Explanation

Synoptic description. Functional explanation.—The most elementary form of describing a set of facts is by simple enumeration. A more economical way is sometimes offered by such comprehensive synoptic devices as mathematical equations fitted to the data. These and related summary descriptions of relations define what is known as an "empirical law." Psychologists from Ebbinghaus to Hull and Woodrow (see secs. 15 and 22) have in this manner attempted to establish psychological laws of varying levels of generality.

Subsuming a new observation under a previously observed or hypothesized functional relationship (or arc) may be called "functional explanation." In particular, reference to a terminal focus in functional explanation may be called a "whither"-explanation; backward reference to an initial focus may be designated as "whence"-explanation. If the initial focus is assumed rather than observed, we have what Spence[1] calls a "response inferred construct." The difficulties in making central inferences from responses can be alleviated by proper consideration of the fact that under the definition of behavior such infer-

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ences cannot be made from one trial or along a single media-
tional track but must use the principle of vicarious functioning
(sec. 6); thus varied and repeated accidents may establish a
suicidal "purpose," although the occurrence of a single accident
could not automatically be "explained" in this manner.[35]

Whither-explanations of behavior in terms of needs, wishes, or purposes
are quite similar to those practiced and readily accepted in another macro-
scopic social-biological discipline, economics.[34] An example is the explana-
tion of railroad communication in terms of "transportation needs."

Intervening variables and hypothetical constructs.—In their
theories of behavior, Tolman and more recently also Hull have
spoken of "intervening variables"; mostly these are assumed for
the least accessible, central region in the network of organism-
environment relationships. As was argued by MacCorquodale
and Meehl,[1] an intervening variable in the original sense is "ab-
stractive" in that it refers to a quantity obtained by a specified
manipulation of the values of empirical variables; it will involve
no hypothesis as to the existence of nonobserved entities or the
occurrence of unobserved processes, physiological or other; "it
will contain, in its complete statement for all purposes of theory
and prediction, no words which are not definable either explicit-
ly or by reduction sentences in terms of the empirical variables;
and the validity of empirical laws involving only observables
will constitute both the necessary and the sufficient conditions
for the validity of the laws involving these intervening vari-
ables." By contrast, the authors propose that, wherever the
theory contains a physiological or other "surplus meaning" in
the sense of Reichenbach, we speak of "hypothetical con-
structs." Spence has been skeptical about the value of such sur-
plus meaning in psychological theorizing, while others have be-
come increasingly sympathetic toward it.

"Why" vs. "how." Technological-reductive explanation.—
Functional explanations may also be called "why"-explanations
in contradistinction to the "how"-explanations in terms of
micro-mediational, technological detail (sec. 8). The former
deal with the grand strategy of the organism; the latter, with
the more subordinate aspect of its tactics. Tracing, or hypo-

thetically filling in, intervening processes helps to reduce gross functional relationships to familiar mechanisms and may thus be designated as "reductive" (this reduction to detail is not to be confused with the reduction to observation in operational definitions; see sec. 4).

In our above example of railroad communication, how-explanation is exemplified by references to cylinders and wheels rather than to transportation needs; this corresponds to the emphasis on anatomical or physiological detail of sense processes or nerves in early experimental psychology (sec. 15). Since in historical perspective micro-mediationism seems to have tended to sidetrack psychology from the proper study of focal and vicarious patterns, we may speak of the underlying scientific urge or compulsion as *explanatio praecox*.

In his discussion of operationism, Feigl[1] distinguishes a series of "levels of explanation" from straight description through empirical laws—also called low-level, surface, or phenomenological laws—to theories of a higher order. The latter, being the most reductive, also require the highest abstraction; examples are the Maxwell electromagnetic wave theory or the Einstein theory. The term 'high' is somewhat misleading, since it also may remind one of the opposite, that is, the higher-level-of-complexity, functional laws and explanations of macro-behavior; these are at a relatively descriptive level and thus of the lower type in the terminology of Feigl.

In the continuous transition between description and reduction such theories as those of Hull (sec. 22) may reach the halfway mark between the two extremes. Use of a quasi-physiological or quasi-neurological terminology should not detract from the fact that the terms concerned are almost exclusively used in the sense of what Feigl has called "promissory notes" against observational reduction which in all likelihood will never be cashed. They are thus but an embellishment or a subjective expression of belief in the possibility of ultimate mechanistic reduction. Mechanical models of behaving organisms such as Tolman's "schematic sowbug" or Boring's "robot" are largely achievement-centered and do not go very far into micro-mediational detail.[35]

Ultimate observational reduction of the intra-organismic portions of behavior must always be physiological. Among the possible interests in this direction, the preponderantly central physiological identification of focal variables or dispositions such as intellectual ability or libido appears to be more legitimate from the standpoint of functional psychology than that of peripheral or micro-mediational detail.

"Dynamic" psychologists, with their concentration on the "why" aspects of behavior, such as Woodworth (see sec. 21),

have urged that psychology—just like, say, geology—should not be considered a "fundamental" science in the sense of reductionism. Acceptance of the wisdom of this self-restriction should help the development of the molar approach as a research style in its own right. Such patterns of descriptive synopsis as the lens model would then come close to the level of psychological ultimates (see sec. 11).

III. Misconceptions of Exactitude in Psychology

For a while, psychology has tried to copy not only the basic methodological principles but also the specific thematic content of physics, thus nipping in the bud the establishment of the somewhat more specific methodological directives outlined in Chapter II. As Murchison has put it, psychology has tried to do *what* physics does rather than trying to do *as* physics does. The ensuing fallacies, or "biases," surrounding the concept of objectivity, operationism, or scientific exactitude in psychology may be grouped in three major syndromes. These range from thematic physicalism proper to some more derived preconceptions, hesitations, and inhibitions.

11. Thematic Physicalism

Analogizing and emulative physicalism. The molecular and the nomothetic bias.—To the outside observer, two policies stand out most prominently in the classical physics of Galileo and Newton (or in the high-school physics of our day). The first is given by the attempt to break up matter, at least conceptually, into small particles or mass-points. If cultivated uncritically or at the expense of other aspects of scientific procedure, this may lead to an unduly atomistic, elementaristic, microscopic, or, as the psychologist prefers to call it, a molecular attitude. The second policy is the nomothetic, i.e., law-stating, resynthesis of these particles which culminates in such allegedly singular principles of strict and universal validity as the law of gravitation.

In an effort to do for "mind" what physics has done for "mat-

ter," psychology began to drift into elementaristic sensationism (see secs. 1 and 2). It further attempted to establish the law of association of ideas by space-time contiguity as the sole determiner of the course of thought. (Contiguity-association is best exemplified by Plato's reference to the image of a friend elicited by the perception of a lyre this friend had been observed to play in the past.) The rather naïve transfer of elementarism and nomotheticism from matter to mind, that is, from one of the two "ontologically" contrasting realms postulated in philosophical dualism to the other, may be labeled "analogizing physicalism."

A second subvariety of thematic physicalism, to be called "emulative," is the preoccupation with elements and strict laws in objective behavior research. As does analogizing physicalism, emulative physicalism springs from an overawed "me too" attitude of a psychology struggling in the wake of an outdated image of the older natural sciences. But while the first has fallen short of the requirements of methodological physicalism, the second has by and large managed to become part of unified science, although it has remained unable to sever the umbilical cord so far as thematic identity is concerned. Superficial behaviorization is accomplished by shifting from sensation to the (discriminatory) reflex as the basic element and from association-of-ideas to conditioning (and other associationistic principles of learning) as the one basic law.

Statistics as the resolution of the nomothetic-idiographic dichotomy.—A particular confusion of issues seems to underlie the widespread appeal of the nomothetic bias. As the present writer has pointed out elsewhere,[1] the confusion is one "between univocality of observation and communication, on the one hand, and the univocality of prediction." From the standpoint of methological rather than thematic physicalism, however, a statistical correlation coefficient or any other probability law is just as exact, that is to say, just as public and palpable in its meaning as a strict law, although here exactitude has as its object the uncertainties of life. All the vagueness lies in the behavior, in the organism-environment relationships described

rather than in the researcher or in his scientific activity by which the behavior in question is approached.[36]

The distinction between natural and cultural sciences (*Geisteswissenschaften*) is traditionally linked with that between the strictly nomothetic (and explanatory) approach which in principle claims absolute predictability and the idiographic (case-descriptive) approach which, strictly speaking, does not know of predictability. Yet "clinical" psychology with its emphasis on understanding the single case is possible only by the recognition of a certain self-consistency of the individual throughout shorter or longer stretches of his personal history on a probability basis, and with the tacit or explicit help of a more generalized study of individual differences and their interrelationships. Even the spatially unique topographic aggregates of geography show marked consistency in time and thus deal with "local" probability laws[77] of a statistical character rather than with bits of information strictly isolated from each other. Generally in modern science, the unrealistically absolutized nomothetic-idiographic dichotomy is resolved on the common ground of the probability approach.

The confusion of objectivity with strict nomotheticism seems widespread even among the most sophisticated of exact scientists. Thus the "cyberneticist," Wiener (see sec. 23), in a review of Philipp Frank's recent book on the philosophy of science,[28] critically characterizes the notion of probability as one "which is never a perfectly operational notion." In criticizing the present writer's probabilistic functionalism, Hull[1] has maintained that it entails a doubt in the "existence" of laws of behavior. However, espousal of the probabilistic point of view in psychology implies that the term 'strict law' ceases to be part of the grammar of the specialized language dealing with the functionalistic aspects of gross behavior. It is no longer appropriate to investigate strict laws in psychology; their repudiation is a methodological or representational, not an ontological, statement. It is related to real skepticism about lawfulness of behavior in the same manner as is the methodological statement, "consciousness is not an adequate subject-matter of scientific inquiry" (as guardedly espoused in this presentation) to its naïvely metaphysical alternate, "consciousness does not exist" (as sometimes found, alongside with the first-mentioned version, in the writings of classical behaviorists such at Watson). The compatibility of physical law and correlation-statistical approach in psycho-

logical ecology has been concretely demonstrated by the writer[30] (1947, Fig. 10).

The spell of the nomothetic ideal has in certain respects extended from the natural sciences to the *Geisteswissenschaften* and their attempted application in psychology. In the so-called "understanding" psychology of personality the coherent architecture and self-consistency of the individual are idealized to the point of suggesting logical deductibility of traits (*Wesensschau*, see sec. 17). Since at the same time the uniqueness of the individual or "existential type" is also idealized, one may be tempted to characterize this approach as "idionomothetic." Here too a statistical damper will have to be applied.

The sociological and ethical roots of the nomothetic idol of the natural sciences (and of its derivative, mechanism; see below), and the increasingly descriptive emphasis in modern physics, has been pointed out by Zilsel (this *Encyclopedia*, Vol. II, No. 8, pp. 61 ff., 90 ff.).

Quantification and topological language.—Since the time Descartes had alleged the nonspatiality of "mind" as the "thinking substance" in contrast with the "extended" character of "matter," the intrinsic nonquantifiability of sensation and of other conscious "qualities" has remained an almost unchallenged axiom. On this account, Kant expressed doubt in the possibility of a scientific psychology altogether, whereas Herbart, although devising an elaborate system of hypothetical quantitative laws, went along to the extent of doubting the possibility of experimentation in psychology. Criticism of the first quantitative psychological law, the Weber-Fechner equation stating the proportionality of "sensation" to the logarithm of the stimulus, was also largely based on this "quantity objection."

Ever since Hering, however, phenomenologists have insisted that there is a "visual space" in which experience is organized; they and the gestaltpsychologists have described the inhomogeneity and "anisotropy" of this perceptual, or "behavioral," space as compared with the physicist's space. Such recent investigators as Stevens[3, 23] have pointed to the surprising consistency and to what may be called the "transitivity" of such introspective operations as the bisecting or doubling of subjective auditory intervals. Facts of this kind tend to mellow the absoluteness of the quality-quantity dichotomy (see also sec. 4).

The major argument against the quantification bias in objec-

tive psychology is based on mathematics itself. As has been pointed out by Russell (this *Encyclopedia*, Vol. I, No. 1, p. 41), measurement and the use of numbers go beyond the essential requirements of the objective approach as defined by univocality of observation and of communication. Relational terms such as 'between,' 'above,' 'ancestor,' designating order in a series, is all that is required. The usefulness of such "comparative" or "topological" concepts has also been pointed out by Hempel and Oppenheim.[81] Psychological statistics makes extensive use of them in such techniques as "rating" and "ranking." The nature of psychological scales has been discussed by Stevens and by Coombs.[37]

Reductive micro-explanation and mechanism. Organic field theory vs. machine theory. Emergence vs. vitalism.—The ultimate ideal of micro-explanation is "mechanism," traditionally defined as reduction of gross functional relationships to the known basic principles of physics and chemistry. In dogmatically insisting on mechanism without sufficient means of empirical proof, the classical behaviorist confuses the unity of science in terms of the requirement of univocality of observation (methodological physicalism, sec. 4) with the unity of laws as we proceed from inanimate to biological phenomena (see Brunswik[1]). As we had to reject another confusion, that between the univocality of observation and the univocality of prediction given by strict vs. probabilistic laws (see above), physicobiological unity of laws must likewise not be taken for granted in an a priori manner in the framework of the objective sciences. As was pointed out by Carnap (this *Encyclopedia*, Vol. I, No. 1, pp. 60–61), the compatibility of biological or psychological laws with, or their derivability from, the system of physical laws cannot be inferred from the reducibility-of-terms but must be left to an empirical investigation. With reference to what has been pointed out concerning the statistical character of psychology, one may add, with Nagel (this *Encyclopedia*, Vol. I, No. 6, p. 24), that "the use of probability statements requires no commitment, even by implication, to any wholesale 'deterministic' or 'indeterministic' world-view."

Assertion of ultimate dualism of explanatory principles is called "vitalism." As an empirical problem, the mechanism-vitalism alternative is, like all problems of reductive explanation, a molecular rather than a molar problem. Its treatment requires minute control of energy exchange and of the technology of such biological phenomena as self-regulation, restitution, or regeneration. If an approach to it is at all feasible, it is a matter for the biologist proper, not the psychologist. Present-day behaviorists such as Tolman, Skinner, and others have therefore tended to ignore the controversy, although there can be little doubt that as scientists at large they expect ultimate mechanistic reducibility of behavior.

Any empirical verification of mechanistic or of vitalistic statements would require naming the concrete operations involved in testing them. Whenever such procedures are not given, or when the hypothetical entities or forces are explicitly declared to be unobservable (as in the case of Driesch; see Zilsel, this *Encyclopedia*, Vol. II, No. 8, p. 77) or even to be nonspatial—somewhat in the manner of Descartes's spaceless *res cogitans*—vitalism ceases to have scientific meaning and becomes a "metaphysical" pseudo-problem, that is, one dealing with assumed entities incapable of operational verification or falsification.

To be distinguished from vitalism is the emphasis, on the part of gestalt-psychology and related movements, upon interaction of physical forces in a molar, dynamic context. "Field" theories (see secs. 10 and 18) are contrasted with the more "machine-like" explanatory schemes supplied by the traditional reflex-arc or association theories. Both types of theory are physicalistic in an explanatory sense, i.e., naturalistic, and thus in the wider sense mechanistic, although the field theories do not subscribe to the particular mechanical models suggested by the machine theories. The terms 'organismic' or 'wholistic,' in the past sometimes applied to vitalistic conceptions, are increasingly being used as a synonym for 'molar.'

Vitalism must also be distinguished from what is becoming known as "emergentism." In proceeding from inanimate to biological and sociological occurrences there appears to be, on the descriptive level, an increasing "novelty" of structures and laws. Although in this sense the complex patterns do not seem to derive from the simple, no special explanatory principles may be needed. As has been pointed out by Feigl, even such a basic phenomenon as the law of gravitation requires a certain complexity of scope to make it discoverable, to wit, the taking into consideration of two rather than just one body. One body, when related to a frame of reference, would merely suffice to state, say, the law of inertia. Similarly, the

kinetic theory of heat is based upon statistical hypotheses superimposed upon, and not anticipated in, Newton's mechanics, without actually transgressing the framework of mechanism in the broad sense of the word (see Frank, this *Encyclopedia*, Vol. I, No. 7, p. 26). In this sense such structures as the lens pattern (secs. 6 and 8) may turn out to be properties emergent on the molar level of approach; very likely they may be fitted into the physical conception of the universe which no more difficulty than the somewhat less complex physical principles just mentioned.[38]

The above example of gravitation as an extremely low-complexity case of "emergence" is attributed by Feigl to a casual conversational remark by the physicist, Bridgman; the obvious intent was to caricature the efforts of some emergentists posing as vitalists. In a more serious vein, Feigl stresses that there are no frames of reference without mass of their own that would lend themselves to the determination of the inertial movement of the body in question. His example was merely to bring out the fact that even in phenomena which are generally considered non-emergent, one could by sufficiently narrowing the basis of evidence make them emergent.

As has been pointed out by Bertalanffy,[39] the recent extension of thermodynamics from closed to open systems, advanced especially by Prigogine since 1946, has led to "fundamentally new principles" under which "self-regulation" and "equifinality" can be subsumed. Especially equifinality—given when the "final state may be reached from different initial conditions and in different ways" in the sense of our "terminal focusing" (see sec. 6)—has often been considered the main proof of vitalism. As the author points out, equifinality is indeed impossible in closed systems; it is for this reason that equifinality is, in general, not found in inanimate systems. Organisms have in the past erroneously been likened to closed systems with their tendency toward equilibrium; actually they are—as had already been pointed out by the biophysicist, Hill, in 1931—open systems in a relatively time-independent, quasi-stationary "steady state" maintained by a continuous flow of component materials. This "dynamic" equilibrium is usually very far from static equilibrium, although its net result may appear to be an absence of reactions in the normal state. Bertalanffy further explains that "according to definition, the second law of thermodynamics—the law of increasing chaos or 'entropy'—applies only to closed systems, it does not define the steady state." In this sense classical thermodynamics is not a consummate doctrine but rather "fragmentary"—as fragmentary, we may add, as a perception or learning theory that ignores distal reference. Open systems may thus "spontaneously develop toward states of greater heterogeneity and complexity" in accordance with a principle that is inherent in physical processes and that is "new" or "emergent" only in the sense that it requires an expansion of scope in order to become discoverable. The "supposed violation of physical

laws does not exist, or, more strictly speaking . . . it disappears by the extension of physical theory." Basic developments in physical theory may in this manner be instigated by biological considerations.

In a similar vein, Schrödinger[40] points out how the organism is continually extracting orderliness, or "negative entropy," from outside to counterbalance the increasing disorder toward which the organism would degrade as a closed system. This happens in a manner perhaps surprising but "not alien" to physics; it constitutes just as integral a part of physical science as the classical laws.

Should the structural characteristics of behavior stressed so far in this presentation, especially that of equifinality, prove too unspecific for its complete explanation, the search for reduction models other than the lens analogy (Chap. II) would have to start afresh. On principle, the outcome of such a search is always uncertain. But it is encouraging to know that efforts at eventual resolution have thus far never remained unrewarded.

Misunderstandings of the principle of parsimony.—The striving for a unified theory of behavior in Watson, Hull, and others is sometimes done in the name of the principle of "economy of thought." In the search for simplicity it is often overlooked, however, that the principle of parsimony applies most properly to "tautological" systems of description, i.e., to systems which describe the same body of objective observations in different ways, and perhaps also to ultimate, reductive explanation, rather than to the relatively macroscopic phenomena of learning or of behavior in general. Furthermore, our choice of the simpler of two logically equivalent theories—such as of the heliocentric in preference to the geocentric view of the universe, or of the relativity theory in preference to classical physics augmented by "additional hypotheses"—merely follows convenience, and none of the rivals can have greater claims as to reality adequacy or "truth" than the other.[28] An analogue to this in psychology is the rotation from one system of axes to another in factor analysis (see sec. 20).

Most controversies in psychology, including that between the association and the gestalt-completion theory (sec. 18), contain differences of opinion about fact or are based on different selection of fact altogether and thus cannot be resolved by merely altering the representation of fact. The relation of association to completion theory has been compared to that between the

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theory of gravitation and electromagnetic field theory; as has been pointed out by Spence,[1] however, there is little intrinsic similarity between mechanics and associationism, on the one hand, and electromagnetic field theory and gestaltpsychology, on the other. In physics, the duality of gravitation and electromagnetism has happily been kept in suspense up to recent attempts at a "unified field theory"; the latter combines the two principles, rather than eliminating one of them at the expense of the other. There is no reason why the plurality of gross dynamic principles in psychology should not likewise be left to remain unresolved indefinitely unless there is empirical disproof of presently claimed observation. (Concerning Lloyd Morgan's version of the "canon of parsimony" see the next section.)

12. Fear of Preliminaries

The iconoclastic bias. Operational redefinitions.—Revolutionary movements like behaviorism tend to develop in an emotionally overdramatized fashion, raising fears of the past in a variety of ways. A comparatively minor way of severing traditional ties is the elimination of palpable verbal symbols associated with older schools of thought. In early behaviorism there was fear that we may be slipping back into "medievalism" (Watson) when employing terms with an objectionable past.

All too often, however, replacement of terms merely leads to highly "visible" yet timid neologisms rather than to a true reconsideration or revision of previous notions. Furthermore, heuristic advantages to be gained from the "apperceptive mass" attached to traditional terms are unnecessarily renounced. Finally, there are usually numerous small and slow changes of meaning in developing a concept ("successive redefinition," see Lenzen, this *Encyclopedia*, Vol. I, No. 5, pp. 44 ff.; Bloomfield, *ibid.*, No. 4, p. 47; Ness[11]), witness the gradual physicalization of such multifacet vernacular terms as 'warm,' 'work,' or 'clock.' If we were to insist on relabeling the maturing concept at each turn of its meaning, considerable cumbersomeness and inflexibility would have to be expected. This would be especially true

in the behaviorization of introspective or speculative terms now getting under way in psychology.

So far as the doctrinaire rechristening of psychology or of its basic concepts is concerned, Bekhterev's "reflexology" or Watson's "behaviorism" are some of the earlier examples. Their effect was, at least in the beginning, the establishment of a "school" set off against the rest of psychology rather than the revamping of psychology as a whole. The term 'behavioristics,' originally suggested as a substitute for 'psychology' by Neurath, has gained some foothold lately, although likewise with a limited field of application (see Boring[50]).

According to Carnap, both in psychology and in the methodology of science terms like 'observation,' 'thinking,' 'doubt,' 'understanding,' 'emotion,' etc., are practically indispensable. And they are admissible (1) as explicanda, i.e., for rough use, in great numbers, and (2) as explicata after redefinition, in a limited number of selected cases.

Psychological objectivists have in recent years increasingly made a point of ostentatiously re-using originally introspectionistic or even vitalistic terms such as those just mentioned. Examples are the operational redefinitions of 'purpose' and 'demand' (Tolman[74]; see also sec. 20), of 'hypothesis' (Krech[41]), of 'intentionality' (Brunswik[7]), of 'faculty' (Thurstone[81]; see also sec. 20), or of 'anxiety' (Estes and Skinner[41]). All these cases are characterized by an effort to preserve the objectified meaning as much of the fruitful conceptual ramifications and connotations of the old concept as possible. This productive halo may have been hidden behind the mask of metaphysics or introspectionism and may have been temporarily lost in the insensitive hands of the classical behaviorist with his predilection for dogmatically peripheralistic translations (see sec. 13).

The anti-introspectionistic, anti-speculative, and anti-philosophical biases. Reinterpretation vs. flat rejection of metaphysics.—A special case of relinquishing imperfect modes of approach regardless of their potential fruitfulness is given by the early behaviorists' over-all rejection of introspectionism, and of speculation and philosophy in general. Aside from the fact that the "private" character of conscious data is a relative rather than an absolute matter (see sec. 4) and the line between science and not-science may therefore be difficult to draw, subjectivistic philosophy and introspective psychology is proving itself time and again an inexhaustible nursery for the supply of novel problem patterns and for reorientation regarding the basic aims and outlook of psychology. The best illustration of this

fact is the reoccurrence, in objective psychological research, of patterns first conceived of, decades or even centuries earlier, in the more flexible medium of philosophy or of introspective psychology and developed there up to the limit of the potentialities inherent in that medium (see Chaps. IV and V).

In many such cases of parallelism of the subjective and the objective approach, stimulation may have been implicit and unnoticed in slowly maturing "climates" of opinion or of interest. There also may be little overt terminological resemblance between the different approaches. No scientific problem may be confounded with its predecessor(s) in metaphysics, of course; but from a genetic-historical point of view many scientific problems appear as the more mature, adult stages of philosophical controversies, thus reaping the harvest of earlier frustrated arguments. It is in this sense that psychology may be said to be the legitimate heir of a good part of philosophy. Accordingly, there are increasing signs of rebellion against the "armchair taboo" in psychology.[42]

An ontogenetic parallel to certain aspects of the history of ideas is furnished by an experiment on the development of criticism in children.[43] Initial total rejection of objectionable statements—much in the vein of the negativism of early behaviorism—was found to be followed by specified rejection and/or positive rectification of the statements concerned, and still later by "higher forms of criticism." These latter include "productive reinterpretation," that is, the assignment of a new, figurative meaning which could be accepted as true, and "social reference," consisting in an effort to understand the statement in the light of the special external or internal conditions under which its author was assumed to have made it.

The general parallelism of the child's conception of the world with that of ancient philosophers has been pointed out in striking detail by Piaget.[44] Like Aristotle, ten-year-old children tend not to recognize the principle of inertia in their efforts to explain the flight of a projectile. Smaller children fail to consider still more elementary principles of "conservation," such as that of number when spatial arrangement is changed. The nature of scientific thought as a maturation process with an inherent sequence of necessary yet imperfect stages thus becomes the more evident.

In certain respects, such as in the undue lingering of introspectionism well into the twentieth century under the influence of dualistic philosophy, one may concur with Philipp Frank's statement that "the content of yesterday's metaphysics is today's common sense and tomorrow's nonsense" (this *Encyclopedia*, Vol. I, No. 7, p. 10). Yet, the balance sheet of metaphysics as a stimulant to psychology is probably definitely on the positive side. Metaphysical antitheses, such as especially that of empiricism versus rationalism, stand behind a host of scientifically meaningful controver-

sies, among them those of sensationism versus phenomenology (secs. 1 and 2), of Helmholtz' empiricism versus Hering's nativism in the theory of space perception,[23] of association (conditioning) versus the internal field dynamics of closure (sec. 18), and of behaviorist environmentalism versus hereditarianism (secs. 13 and 18). To paraphrase Feigl,[1] it is only the "negativism" inherent in any false perfectionism that will cling to the letter of metaphysical formulations, readily dismissing them as pseudo-problems. A true "positivism" realizes and constructively develops their inherent potentialities.

Descriptive vs. vitalistic teleology. Anthropomorphism vs. animism.—Reference to terminal foci (see secs. 6 and 8) is sometimes being called "teleology," especially by its critics. However, terminal reference was found to be an essential requirement of the functional approach so long as it remains purely descriptive or is used only for the functional type of explanation. An objectionable form of terminal reference is given only in the case of micro-explanation in terms of dogmatically assumed extra-natural, vitalistic principles of a metaphysical kind (see sec. 11).[45] Deliberate and extensive use of the term 'teleology' in the description of calculating machines and related mechanisms has recently been made in "cybernetics" (sec. 23).

Historical affiliation of objectivistic and anti-teleological tendencies in psychology is revealed by the fact that Lloyd Morgan, in his criticism of the "anecdotal" type of animal psychology prevalent in the nineteenth century, conceived of the objective approach primarily as one that keeps description at the lowest possible level (canon of parsimony) and avoids "anthropomorphistic" interpretation of animal behavior. Metaphysical-explanatory anthropomorphism, or "animism," is indeed objectionable; the operational anthropomorphism of the molar behaviorists, on the other hand, has been vindicated at the beginning of this section.

13. Hostility to Theory and to Central Inference

The dubious reputation of introspection and philosophical speculation with the objectively minded psychologists of the turn of the century also led to a tendency to throw overboard all theorizing. This was undoubtedly aided by antitheoretical movements within the physical sciences of the immediately preceding period as exemplified by Mach's aversion against such inferential constructs as the atom.

The observational bias. Emphasis of "data."—Early American functionalism and Watsonian behaviorism urged restriction of psychology to observed "facts." In this they followed in the footsteps of Comte, whose early "positivism" in many ways anticipates operationism and behaviorism, and who uses 'positive' as a synonym for 'observational,' 'preinferential,' 'undebatable.' Actually, the blind gathering and intercorrelation of data has led to selective emphasis upon regions relatively accessible to observation, thus sacrificing relevance to convenience, and has opened the door to tacit presuppositions of a more theoretical kind as well.

Köhler[46] has described this attitude as the outgrowth of a general "philosophy of caution." One may add that any safety-first approach is bound to be "inhibitory" rather than "excitatory," constrained rather than relaxed, intellectually puritanical rather than venturesome in a "Dionysian" way; it is genocidal in that it polices the very seeds of imagination and thought which Bridgman,[47] his operationism notwithstanding, came out to defend as the necessary "private" component in the establishment of science.

Peripheralism. "Glorification of the skin."—In consequence, Watson tended to "belittle the brain and the central nervous system" (Heidbreder[50]) and to shift the "definition" of even such cogently central activities as emotion and thinking to the sensory or motor periphery. Practical accessibility thus became confounded with importance, in a manner aptly criticized as "glorification of the skin."[4]

By dogmatically translating 'thinking' as 'laryngeal habits' or 'implicit verbalization' (for discussion see Murphy[50]) Watson's behaviorism became the objectivist counterpart of medieval nominalism, and of English empiricism and structuralism with their assumption of sensory (pictorial or verbal) images as the constituents of thought. This is in sharp contrast with the "conceptualist" leanings of the Würzburg school with its emphasis on an independent representation of the abstract and general in thinking (sec. 2); over and beyond this, vicarious functioning of alternative verbal "formulations" of identical thoughts has been experimentally demonstrated for memory as well as for thinking proper by Karl and by Charlotte Bühler.[48] All this has helped to pave the way for the centralism of the molar behaviorists (sec. 20).

Physiologism as an escape from functionalism.—Next to the establishment of the peripheral anchors of behavior, the observational step-by-step tracing of mediating physiological processes has also attracted the attention of classical behaviorists as well as of such general theorists as Pratt,[49] mostly with an eye on the nomothetic micro-explanation of behavior.

The more remote ("distal") stimuli that precede, as well as the more remote effects, products, and permanent institutions that result from, behavior proper are likewise plainly measurable and are often accessible with great ease. Characteristically, however, classical behaviorism has made little use of these opportunities, although most of its representatives have paid lip service to them. Among them is especially Bekhterev,[70] who wishes to study "the complete correlation of the organism with the external world," may the latter be close or remote in space or time. The prime reason for this neglect must be sought in molecularism rather than in observationism. The functional-behavioral units studied by the inclusion of distal variables bridge over arcs too long for comfort, and they seem to provoke the dreaded teleological description.

Associationism and environmentalism.—It is likely that the relatively direct origin of contiguity association and conditioning in external chains of events had much to do with the appeal of this principle to empiricists of the more restricted type. Related to this is the emphasis on environment rather than heredity, on "nurture" rather than "nature," on learning rather than maturation,[69] in classical behaviorism. Again, there is reluctance to acknowledge factors which are not directly controllable, or so removed or otherwise unknown that dealing with them could leave loopholes for rationalistic "innate ideas" or vitalistic "instincts." The existence of a well-defined "anti-instinctivist" movement within classical behaviorism supports this interpretation.

IV. Traditional Approach and Constructive Crisis in Psychology

14. Historical Schema and Interpretative Hypotheses

Objectivity, molarity, regional reference.—The subsequent historical analysis[50, 51, 52] will be in terms of the three basic aspects developed up to section 8: (1) objectivity; (2) molarity, involving both the functional arc and the width of vicarious mediation; and (3) regional reference, involving the distinction of central, peripheral-proximal and distal areas, both on the reception and on the effection side and, as will be seen, both along the situational and along the historical axis (Figs. 2–10).[53]

Five working hypotheses.—The following interpretative hypotheses are offered and will be explicitly or implicitly considered during the remainder of our presentation:

1. There seems to be a continuous change-over from subjectivism to objectivism in psychology (from left to right in the main table containing Figs. 2–7, the first two of which were already discussed in secs. 1 and 2).

2. Within each of these two avenues of approach there seems to be a development from an emphasis on confined core events through peripheralism to functionalism, that is, broadly, from a static and molecular to a dynamic and molar type of approach (from top to bottom in the table).

3. Progress along one of these two perpendicular directions seems frequently accompanied by a standstill or even a regression along the other, but in the end this standstill turns out to be only temporary.

4. The successive subjective phases appear as encapsulated, relatively casual miniature anticipations of the reference patterns found in their physicalistic counterparts (connected with the former by slanted lines in the table), the latter culminating in a central-distal functionalism.

5. There seems to be a time lag between these formally corresponding introspectionistic and objectivistic stages which decreases as history proceeds, perhaps from as much as a few cen-
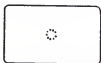
PURE INTROSPECTIONISM:
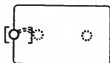
Fig. 2. Meditative philosophy
(From 17th century)

Areas (regions):
Geographic (ecological) environment

Distal
Proximal
Peripheral
Central
Stimuli
Organism
Overt responses

OBJECTIVE APPROACH:

Fig. 3. Sensotionism
(18th and 19th centuries)
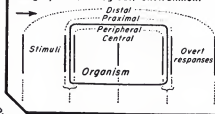
Fig. 5. Micro-physiologism
(First half of 19th cent.)

Fig. 4. Intentionalism
(Last quarter of 19th cent.)

Fig. 6. Classical behaviorism
(First quarter of 20th cent.)

Process reference

Peripheralism

Functionalism

"Empty organism" approach

Thing-constancy

Motivation, attitude

Motor behaviorism
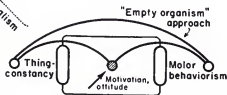
Fig. 7. Objective functional psychology
(Second quarter of 20th cent.)

Symbols (for all figures):

	Observed	Intraspective	Objective
Units, variables, processes			
Relationships			
(Univocal rel.)			

Inferred processes and hypothetical univocal relationships

Tacit presuppositions

Figures 2 to 7. Major stages of introspective and objective psychology

turies at the beginning to several decades in the more recent stages (compare chronological references given with Figs. 2 and 5, 3 and 6, 4 and 7), so that in many cases there is a spiral recurrence of analogous principles on more advanced levels of methodological perfection (see sec. 19).

15. Sensory Psychology

Micro-mediational physiologism.—A strong impetus for the development of experimental psychology comes from the biological and medical sciences, among them most directly from physiology. Physiological problems possessing psychological implications are best exemplified by the studies of Johannes Müller and of Helmholtz on the speed of nervous conduction. Intra-organismic, micro-mediational process interest of this kind is graphically represented in Figure 5 by a straight line placed well within the boundary of the organism.

Other problems conceived in the general manner of micro-physiologism are the brain localization of peripheral—sensory and motor—functions, as well as most aspects of Helmholtz' sensory anatomy and physiology. The latter deal with the short-span relationships between proximal stimulation and peripheral physiological excitation and with the technology of neural transmission as schematically represented in Figure 8.

Stimulus-response psychophysics, physiological psychology.— The declared core of the classical approach in psychology is structurally to analyze, inventorize, and classify the basic elements of consciousness, as briefly described in sections 1 and 2. Auxiliary to the study of consciousness as such is (1) the finding of its external stimulus correlates—the psycho-environmental problem of Fechner's "psychophysics" (1860)—as well as (2) of its physiological counterpart, in most cases under the basic assumption of psycho-physiological parallelism (sec. 11).[54]

Psychophysics is diagrammatically described in Figure 8. Interest centers about a functional relation which, although short-arc and thus elementaristic by comparison with later types of research, establishes the principle of studying a gross stimulus-response relationship, while problems of sensory and nervous

technology are removed to a secondary position. One of the terms of the relationship, here the response, is conceived of as a conscious content, sensation; verbal or other expressive behavior is interpreted as a tool of introspection. The programmatic outlook of classical psychophysics is thus physicalistic with respect to the stimulus, while it remains mentalistic concerning the response; in short, it proposes to be S-objective, R-subjective. Since stimulus-response correlation is under strictly nomothetic aspects without realization of the principle of
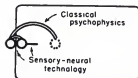

Fig. 8. Sensory psychology
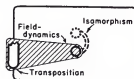(Second half of 19th cent.)

Fig. 9. Gestalt psychology
(First quarter of 20th cent.)

Figures 8 and 9. Part-objective stages of experimental psychology

vicarious functioning, a double arc is used in Figure 8 to represent the basic quantitative psychophysical "law" proposed by Weber and Fechner (see sec. 11; the double arc may at the same time be taken to represent the "constancy hypothesis" in a somewhat different manner in Fig. 3).

Introspectionism, elementarism, sensationism, associationism. —A further nomothetic contribution is Ebbinghaus' study of memory for nonsense syllables (1885). By this study, contiguity association is brought within the orbit of psychological experimentation. This completes the picture of nineteenth-century psychology as an intersection of introspectionistic, elementaristic, sensationistic, and associationistic tendencies (Boring,[50] Bühler[53]).

16. Intentionalism, Early American Functionalism, Psychoanalysis

The second major period of psychology proper has been characterized as an abundance of divergent new growth, all in protest against one or the other major feature or combination of

features of classical experimental psychology. Bühler[55] has spoken of this phase as the "constructive crisis" (*Aufbaukrise*) of psychology.

Intentionalism.—Brentano[56] has prepared the shift from sensationism to phenomenological introspection (sec. 2) by emphasizing the dynamic component of such psychological "acts" as perceiving or judging over their static sensory "content." The focus of introspection thus moves back to the central region where it had been with the meditative philosophers. However, there is a new stress on "directedness toward objects"; these objects are conceived as "inexistent" in mind (in the sense of momentary consciousness at large). Brentano has raised this "intentionality" to the major criterion of the subject matter of psychology; William James[57] has made it one of the chief characteristics of his "stream of thought."

Brentano's "objects" are not to be confused with the physicist's constructs, or "reality," which likewise are sometimes said to be immanent or inexistent in consciousness. In the related "*Gegenstandstheorie*" of Meinong anything our thinking may be concerned with as a problem, whether in reality possible or impossible, such as a round square, is called an object. Russell has criticized intentional objects as establishing an ontological "third Reich" that is neither mental nor physical. Uexküll and Bock[79] speak of the "monadological" introjection of the organism-environment relationship into the subject. Brentano himself hesitated to assign to the intentional relationship more than "quasi-relational" status (using the term '*Relativliches*' rather than '*Relation*'). This terminological trick constitutes recognition, on his part, of the fact that his intentional objects lack anchoring in what is now called an "independent approach," that is, the measurement and correlational scrutiny of the proximal or distal stimulus variables attained by a certain type of perceptual response (see sec. 20); indeed, intentional objects have their sole basis of observation, if not in analytic introspection, then in an encapsulated phenomenology or other revised form of introspection or retrospection.

In fact, the distinction between intentional objects and per-

ceptual contents or acts can hardly be maintained save for some comparatively subtle differences in introspective tone. In Figure 4 the directly experiential character of the intentional objects is indicated by the use of dotted circles pointed at by dotted arrows (for intentionality as a relation), especially the one to the left; content, act, and object are inclosed within a dotted oval which is to represent the basic schema of dramatization in intentionalistically conceived central time-slices, conscious or dispositional, of which Lewin's "psychological life-space" (sec. 22) is another example.

Early American functionalism.—The encapsulated introspectionism characteristic of pure intentionalism was counterbalanced, toward the turn of the century, by the functionalism of Dewey, Mead, Angell, Carr, and others at the University of Chicago.[58] In criticizing the traditional molecular physiological emphasis on the reflex arc, Dewey[52] saw the key to the unity of psychological activity in its "function" or "successful issue," the "organization of means with reference to a comprehensive end." Reference to the region of distal results was thus established along with a utilitarian, adjustment-centered biological conception of psychology which may be traced to Darwin's views on the struggle for existence. (Concerning the "law of effect"[59] see sec. 18; on the recent expansion of functionalism in America see Chap. V.)

Psychoanalysis as depth-psychology.—Beginning in the 1890's and headed by Freud,[60] the psychoanalytic movement challenges the ordinary, controlled type of consciousness as the true representative of motivational dynamics. While in this sense psychoanalysis is definitely anti-introspectionistic, it shows partial adherence to conventional introspective methods by its relying on verbal reports of dreams or of the purposely disinhibited stream of ideas ("free association") as some of its "manifest" data. Peripheralism definitely is given up in favor of the more central emphasis on motivation. Associationism is challenged in that, in the eyes of psychoanalysis, dreams and the so-called free association is neither contiguity association nor free; rather, the course of ideas under disinhibition is considered to be

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largely governed by "repressed" instinctual tendencies, or "wishes," mostly in symbolic disguise. As is also true for gestalt-psychology (see sec. 18), the anti-associationism of psychoanalysis is not complete, however; contiguity association and such secondary principles of classical association theory as "recency" (experiences of the preceding day) are conceded to be modifiers in the specific choice of dream symbols. Although a few of the acknowledged symbols (e.g., water as a symbol for birth) are traced to space-time contiguity, most of the sex symbols are based on formal, geometrical similarity.

Reference to wishes and instincts as prime movers of the course of ideas and of behavior constitutes an element of intentionalism in psychoanalysis. Acquaintance of Freud with the intentionalism of Brentano has recently been definitely established.[61] Although attempts at reductive physiological identification of instincts or wishes are infrequent in psychoanalysis, the suspicion of vitalistic (animistic) leanings is unfounded. There is occasional explicit theorizing by Freud "in the spirit of 'physicalistic' physiology."[61]

In the earlier phases of psychoanalysis manifest introspection, as well as both proximal and distal behavior (Murray's[84] "actones" and "effects"; see sec. 21), were used primarily as a basis for inferences regarding the underlying "latent," largely repressed motivational depth stratum and its disturbances, as well as for inferences regarding the historic-genetic background of these disturbances, which was mostly assumed to be found in early childhood (Fig. 10). As was pointed out by Frenkel-Brunswik,[62] "concentration of scientific effort upon the central was not truly attempted before psychoanalysis. Correspondingly, psychoanalysis has, especially in its beginnings, comparatively neglected the distal results and achievements of behavior." In the same vein, Freud's daughter Anna[63] had bemoaned the fact that in early psychoanalysis the value of the scientific and therapeutic work done was seen to be in direct proportion to the "depth" of the psychic strata upon which attention was focused. She points out that whenever interest was transferred from the deeper to the more superficial psychic

strata, from the id to the ego, it was felt that here was a beginning of apostasy from psychoanalysis as a whole; with problems such as that of the adjustment to the outside world, with health and disease, psychoanalysis was not properly concerned. The approach of early psychoanalysis is in this sense fragmentary. So far as the unit of behavior suggested in Figure 1 is concerned, depth-psychology is limited to the initial half; this is elaborated in the right-hand part of Figure 10.
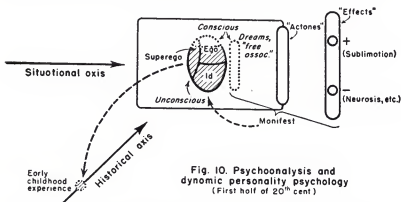

Fig. 10. Psychoanalysis and dynamic personality psychology (First half of 20th cent.)

Vicarious functioning in psychoanalysis.—While dual focusing and the functional arc are thus neglected, a further basic feature of the lens model of behavior, vicarious functioning, has found such sweeping and unparalleled recognition in psychoanalysis —and in the crucial medium of the expression and satisfaction of drives at that—that this recognition may well be considered the most important contribution of this school to psychology.

As was first pointed out by Frenkel-Brunswik,[62] in an effort to integrate psychoanalysis with academic personality psychology under the auspices of logical empiricism, "no process of interpretation . . . takes surface phenomena at their face value. It rests, rather, on 'minimal cues.' It embraces a wide variety of circumstantial evidence—as furnished, for instance, by the techniques of psychoanalysis (free association and transference). A motive is considered central only after it has been recognized in a large number of behaviorally, or distally, diverse or even opposing features revealed in succession (such as an alternation between aggression and over-protectiveness)—if only these features have in common the same symbolic value empirically established by generalized evidence. . . . Psy-

choanalysis . . . has shown a great resolving power in conceptually bringing together an apparently chaotic variety of behavioral features relevant to life. It was in the service of this process of reduction that such important mechanisms as repression, denial, reaction-formation, sublimation, projection, and displacement were discovered." Most of the other concepts of psychoanalysis likewise refer to such mechanisms, stressing either the variety in the kinds of substitution (among them symbol, transference, condensation, rationalization, hysteric conversion, regression, cathexis, narcissism), or the width and flexibility of vicarious functioning (among them fixation, censorship—a more accurate translation of *Zensur* than the customary personalized form, 'censor'—choice of neurosis, frustration, resistance).

Further subsumable under the realization of the lens principle is the development, in Freud, from early efforts to treat hysterical symptoms directly by hypnosis, to a genuinely psychoanalytic approach aimed at the underlying dynamic core (which may also use hypnosis but in a different manner). Recognition of this core as a repressed wish implies that it is likely to find vicarious channels of expression and thus to create new symptoms unless itself removed. This development is paralleled by a general shift from "symptomatic" to "causal" treatment in medicine at large and by a general trend in psychology from peripheral-phenotypical to central-genotypical emphasis (see secs. 20–22).

As is to be expected from the wide variety of vicarious mechanisms, no single diagnostic cue is likely to have the character of foolproof evidence. Correspondingly, the depth-psychological establishment of motivational dynamics on the basis of concrete manifestations is far from being a process of univocal, atomistic "symbol-hunting"; it rather possesses all the earmarks of the probabilistic inferences so characteristic of recent functionalism (see secs. 7 and 9). It further follows that the range of circumstantial evidence for the theory as a whole must be extensive. Indeed, evidence in support of symbolism ranges from the slow direct uncovering of hidden connections in the process of psychoanalytic therapy, to folklore, myth, and jokes. Projective interpretation has occasionally been extended to metaphysical systems, e.g., in Freud's[64] discussion of Empedocles' love-hate philosophy as a "cosmic phantasy." (Note that 'projection' is applied to what such philosophers as Avenarius have called "introjection," and vice versa.) Analogies from biology at large have also been considered.[65]

On the other hand, the haziness of distal reference in psychoanalysis to which we have referred above may in part be the result of some remnants of a tacit "constancy hypothesis" (sec. 2)—as opposed to the full recognition of vicariousness—in the traditional conception of the drive-to-result correlation, "tacitly assuming that biologically relevant distal results are always due to the corresponding drive [and implying] that central causes

directed toward such distal results will never lead to other effects. The sources of disturbance of the central-distal correlation are, however, so great that . . . there are too many central causes leading into side tracks (such as maladjustment) and blind alleys (such as compulsion symptoms), and [that there are] too many distal results not due to central causes explicitly directed toward them" (Frenkel-Brunswik[62]). In terms of the lens model, pathological reactions, or any "going off in vacuo" of behavior (to use a phrase by Tolman), are conceived as outgoing stray rays due to the imperfection of the "lens" (Fig. 1). The terminal foci are obscured in psychoanalysis unless it can be shown that they become organized into more or less autonomous substitute foci. Actually, a good part of psychoanalysis is devoted to the discovery of environmental conditions of quiescence which play the role of substitute goals or new terminal foci. Aside from neurosis and other channels harmful or neutral to the superordinate aspects of maintenance and propagation of life, there are positive, or at least socially acceptable, outlets, the outstanding example being the "sublimation" of the more primordial drives into cultural or scientific endeavors (Fig. 10). However, the concept of sublimation has remained relatively sterile and vague in the writings of psychoanalysis.[85]

Beginnings of functionalism in psychoanalysis.—In the early twenties, some years after being exposed to the rising functionalistic-behavioristic atmosphere in the United States, Freud shifted his emphasis from the introspectively oriented dimension conscious-unconscious to the more objectivistically conceived hierarchy of three dynamic functions within the person, the id, ego, and superego. As is shown in the central part of Figure 10, this part being borrowed from a textbook elaboration of Freud's own diagrams, consciousness comprises much of the ego and part of the superego. The three dynamic institutions represent, or take care of, respectively, the primary drives, reality relationships, and the culturally imposed conscience. Especially the concept of the ego thus prepares a turn to functionalism; as Murray[62] has put it, the ego is in the main defined by "listing what it *does* (repression, adaptation, etc.)." After pointing out the predominantly depth-psychological orientation of psychoanalytic theory (sec. 16), Anna Freud[63] goes on to explain that at least therapeutically psychoanalysis has always been interested in the "ego," the "reality principle," and its aberrations, and thus has proceeded from the roots to the out-

come of human activity. She lists some of her father's own contributions to ego psychology and develops them in a system of "defense mechanisms." Distal results of behavior are thus becoming a programmatically recognized region of reference in psychoanalysis.

Alternative hypotheses as to primary drives. Level of abstraction.—Freud's original system has often been set in opposition to the alternative systems of Adler and of Jung.[66] Yet Adler's major contribution, the attempt to shift earlier reduction schemes from the sex drive to the desire for mastery and prestige, is merely a change of content. The shift is comparable to Heraclitus' substitution of fire for Thales' water as the basic physical element. Little is changed in the level of abstraction or in the basic textural aspects of our diagrammatic scheme in Figure 10. The same holds for Jung's tracing of historical-genetic lines to cultural or "racial" rather than individual factors.

On the other hand, Adler's emphasis on social drives foreshadows the modern American trend toward situationism (sec. 21). And Jung's generalization of the concept of "libido" which continues Freud's own policy to broaden the concept of "sex" beyond its customary meaning helps to prepare the ground for an eventual treatment of motivation in terms of objectively established factors or related "nonpictorial" constructs removed from direct experience or common-sense behavioral observation (see sec. 20).

17. The Decline of Methodological and Nomological Dualism

Phenomenology, intuitionism, and "understanding" psychology.—The crisis of the metaphysical dualism of mind and matter brought about by behaviorism is paralleled by a crisis of the methodological dualism of introspection and physical measurement brought about, within introspectionism itself, by the frustrations of the phenomenological approach (see sec. 2). Related both to descriptive phenomenology and to Husserl's philosophically oriented "intuition of essences" (*Wesensschau*) is the "understanding" psychology of Dilthey and of Spranger.[67] In stressing its ties with the humanities (*Geisteswissenschaften*),

it is said to be one of "two kinds of psychology"; the other, "explanatory" psychology, is seen as allied with the natural sciences by the use of experiment, statistics, and measurement and is dogmatically belittled as an offshoot of the "economic" rather than the "theoretical" value-orientation. Theories of personality are derived from metaphysical systems of values and related sources, leaving much to subjective empathy and arbitrary speculation.

Although allegedly patterned after mathematical deduction, intuition of essences is in fact camouflaged or submerged induction characterized by sloppy policies of inference and a tendency to premature systematization; it is for this reason that *Wesensschau* is in practice flagrantly ambiguous. Spranger's above-mentioned attempt intuitively to reduce the modes of thought of the exact sciences to the economic value system makes use of vague analogies between the equivalence of units in the process of measurement, and the universal exchangeability of goods in a system of values in which "everything has its price." Countermanding the derogatory intent of this derivation, one could point with at least equal cogency to the affinity of the concept of "natural law"—a style element of the natural sciences intrinsically interwoven with the concept of "measurement"—with the moral code and thus with what Spranger would have to subsume under his "religious" value system. Aside from the fact that for this latter affinity verification in historic-genetic terms seems feasible (for reference see sec. 11), one intuition would here be left to stand against another intuition, much in the way one introspection was found to stand against another introspection in the controversy concerning, say, the pictorial versus nonpictorial character of our thought-experiences (sec. 2).

Objective personality psychology shows increasing readiness to take up the problems raised—although not satisfactorily answerable—by the understanding approach. The success of such operational transformations has reassigned intuition of character structure, along with phenomenology, to its legitimate function as a fruitful and perhaps indispensable propaedeutic discipline to objective study in psychology.

Hormic psychology.—In contradistinction to intuitionism with its methodological dualism, vitalism (sec. 11) may be said to espouse a "nomological" dualism in that it postulates different reduction principles for the laws of psychology than may be furnished by physics. There is a vitalistic tradition in psychology

from Aristotle to Driesch and McDougall.[68] The latter has seen the "instincts" as endowed with a unique driving power of their own, declared to be irreducible to physicochemical causality; hence the term 'hormic' (from the Greek equivalent for 'urge'). In the immediate present, vestiges of psychophysical interactionism are limited to occasional one-sided interpretations of such terms as 'psychosomatic disease,' although it is mostly recognized that no more need be implied than a promissory note for the tracing of behavioral disturbances to micro-physiological or hidden rather than gross organic or overtly palpable causes.

18. Antithetic Divergence: Gestaltpsychology and Classical Behaviorism

Gestalt theory of perception.—Evolving from the Graz school (sec. 3), gestaltpsychology reaches its full impact from the 1910's on in the so-called Berlin school of Wertheimer, Köhler, and Koffka.[69] Both schools recognize vicarious functioning of stimulus patterns in perception which possess intrinsic geometrical or melodic similarity. Since this principle of "transposition" ignores "families" of cues the members of which do not formally resemble each other but are held together merely by association (see secs. 6 and 20), recognition of vicariousness remains limited to one of its comparatively trivial aspects (fragmented oval in Fig. 9).

In the more modern versions of gestaltpsychology the perceptual response continuum is seen as broken up into a discrete series of relatively stable, preferred rallying points (*Prägnanzstufen*) characterized by clear-cut, salient organization or "good" form as defined primarily by geometric-ornamental simplicity and regularity, such as symmetry and closure. Although these crystallizations define positive contributions of the reacting organism to perception superseding and specifying the negative aspects of ambiguity and illusion stressed in the earlier phases of gestaltpsychology, stimulus-response correlation is further obscured (omission of the functional arc in Fig. 9).

In trying to explain the role of the subject in perceptual organization, the Berlin school has attempted to replace the older

mentalistic "production" theory of gestalt as held by Meinong and others of the Graz school by a reductive theory of physiological "field" dynamics (indicated in Fig. 9 by the shaded area between stimulus and response). The general principles involved are developed in Köhler's theory of "physical *Gestalten*." Combined with this is the extension of psycho-physiological parallelism to the perception of form ("isomorphism"). Mach's emphasis on the tendency toward geometrical regularity in closed physical systems approaching equilibrium ties in well with the particulars of the law of *Prägnanz*.

In warning us of projectively ascribing organizational properties of the perceptual response such as figural unity (sec. 3) to the stimulus aggregate ("experience error"), Köhler points out that there is no direct transmission, to the perceiver, of physical gestalt properties present in fellow-organisms or other objects. All unity must be newly constructed in the responder in accordance with his intrinsic, "autochthonous" brain dynamics. The subject is thus seen as basically out of contact with the dynamics of the environment, a fact also revealed by the rejection of the concept of learned "cues" hinted at above.[30] While psycho-physiological isomorphism receives all possible attention, the intricate problems of psycho-environmental (central-distal) stimulus-response coordination are, by both Köhler and Koffka, summarily dismissed by allusions to a vaguely conceived kind of pre-established harmony (or extended isomorphism) between the structural principles of the surroundings and the field dynamics within the organism.[69] The frame of reference of gestaltpsychology remains thus as encapsulated within the organism as was that of classical psychophysics (Fig. 8). The absence of genuine distal reference in gestaltpsychology has been explicitly pointed out by Heider.[73]

Closure (completion) as internal determination of the course of thought and action.—A particularly important development in gestaltpsychology is the extension of the law of *Prägnanz* from perception to thinking, thus evolving a principle set in opposition to contiguity association. In interpreting his experiments with chimpanzees, Köhler assumed that, by spontaneous

dynamic reorganization of the perceptual field under the pressure of motivation, sticks or branches of a tree could be "seen" into the unfilled gap between animal and food. This completion or "closure" would in turn lead to the execution of appropriate action. The problem is thus solved by internal determination without benefit of association based on past experience. This view represents a physicalistic revival of the rationists' "mind," thought to be capable of producing a priori knowledge that had not passed through the senses.

Köhler further points out that the principle of completion tends to produce constructive "good errors" even if it goes astray in a practical sense. Some animals fill the gap between hand and goal visually though not mechanically by pushing out one stick by means of another, thus showing "insight" into the principle of the solution. By contrast, contiguity association is belittled and presented as a potential handicap that frequently leads to "poor errors," such as when the ape brings a box, previously found helpful in the gathering of food hung from the ceiling, in a futile effort to obtain a piece of food placed outside the cage.

Peripheral behaviorism.—Preceded by the "reflexology" of Pavlov and Bekhterev,[70] the early phase of American behaviorism is inaugurated by Watson[71] about coincident with Wertheimer's gestalt classic on apparent movement. Peripheral reference (previously discussed in sec. 13) is adopted from structuralism, the conception of a univocal functional arc from classical psychophysics; both are extended to the right and thus objectified, sensation merely shifting place to become "discrimination" by overt reflex, including external glandular secretion as well as what critics of classical behaviorism have designated as "muscle twitches." The functional arc runs straight from the sensory to the motor, by-passing the central region (Fig. 6).

A basic learning principle, Pavlov's "conditioning," deals primarily with the establishment of a new stimulus (e.g., the ringing of a bell) for a certain response (e.g., salivation) ordinarily elicited by another stimulus, after repeated presentation of the two stimuli in space-time contiguity (left arrow in Fig. 6). A symmetrical case (right arrow) is given when a previously unconnected response is linked to an existing stimulus,

such as in learning, by Thorndike's "trial-and-error and accidental success," say, to press a lever effecting escape in a situation of confinement.[3] Both cases imply a measure of vicarious functioning and thus transcend a strict one-one relationship, but the outlook on the problem of learning remains nevertheless nomothetic (double arc in Fig. 6) in the tradition of thematic physicalism (sec. 11). Both conditioning and trial-and-error learning are but operational redefinitions of, or elaborations on, the traditional association by contiguity in its broader sense, with stimuli and responses taking the place of "ideas" (Thorndike's "connectionism").

Opposition to the law of effect in classical behaviorism.—Trial-and-error and related forms of learning involving responses that may be instrumental in changing the stimulus form the basis of Thorndike's "law of effect." According to this law, contiguity association of the response with satisfaction (or annoyance) is a basic mechanism in the stamping-in (or stamping-out), the "reinforcement" (or extinction), of the motor behavior of which satisfaction is a result. This principle injects an element of distal reference into learning theory in the spirit of the utilitarianism of American functionalism (sec. 16), to which movement Thorndike is related. The stigma of teleology and hedonistic introspectionism attached to the law of effect prevented Watson from accepting it, thus seriously curtailing the scope and level of complexity of early behavioristic research. However, as was recently pointed out by Hilgard,[3] Thorndike was actually ahead of rather than behind his behaviorist critics by having given an irreproachable operational definition of a satisfying state of affairs, characterizing it as "one which the animal does nothing to avoid, often doing things which maintain or renew it."

V. Convergence toward an Objective Functional Approach

19. General Trends toward Realization of Norms

Temporary reciprocity of rigor and scope.—Of the two major crisis schools arising against structuralism and defining the

second phase of modern psychology, classical behaviorism may be said to have overcome the introspectionism of nineteenth-century psychology but to have merely shifted the atomism, sensationism, and associationism to the overt behavioral level without altering its underlying peripheralism. Gestaltpsychology, on the other hand, has played the opposite role of overcoming atomism in its sensationistic and associationistic aspects without touching the basically introspectionistic conception of psychology. This divergent pattern of development may in some observers have reinforced the traditional intuitionist belief in the incompatibility of objectivity and molarity. The two have thus been seen as a kind of conjugate variables, so that any increase in objectivity would imply a "complementary" loss of scope. Yet, the inverse relationship observed soon turned out to be temporary and obviously due only to relatively insignificant psychological limitations inherent in formative stages of development. During a third phase of modern psychology, beginning in the 1930's, there is a "convergence" of the predominantly Anglo-American tradition of empiricist rigor (behaviorism, pragmatistic emphasis on overt action, statistical scrutiny) with the predominantly Continental stress on complexity and richness of scope. This convergence possesses all the earmarks of a genuine "synthesis" of the preceding divergent, mutually "antithetical" movements (to apply Hegel's notions of the dialectics of the creative process). It is not an eclectic intercombination of selected fragments of existing schools; rather, abstract methodological features are united for the first time in formations of striking novelty of style.

From metaphysics to methodological positivism.—Of most immediate interest from the logical empiricist point of view is the decline in the explicit entanglement of psychology with metaphysics, and a rise in what Allport[72] calls "methodological positivism."

His and Bruner's systematic content analysis of leading periodicals[72] has documented the "special lure" of operationism among psychologists. There also is an increasing emphasis on "mental process as a construct" that tends to "avoid the hypostatization of mental process." The employment of

"postulational and geometric methods" likewise shows a marked rise after having become practically extinct during the period of intolerance toward theorizing at the beginning of this century. "Explicit treatment of the body-mind problem" shows a decline, said to reflect a "significant shift in viewpoint. In the earlier literature solutions to the problem were boldly offered in monistic or dualistic terms; today the fashion is to deny the existence of the body-mind problem, the denial being generally effected with the aid of Vienna logic."

Central-distal vs. peripheral reference.—More specifically, recent developments in psychology emerge as a combination of a de-emphasis of the peripheral region and the establishment of a central-distal or at least a central frame of reference which takes cognizance of the predominantly central-distal focusing of behavior itself (see sec. 6).[73]

20. Distal-Central Reference in Molar Behaviorism, Probabilistic Functionalism, Factor Analysis

In this section are presented empirical theories which are implicitly or explicitly probabilistic and which, with the exception of factor analysis, move on an intermediate level of formalization. All of them explicitly include objectively measured distal variables such as stimulus objects and/or behavioral results (e.g., the reaching of food, or the score on a test). Usually, but not always, there is also central reference, either of a direct, physiological or of an inferential nature.

Molar behaviorism.—The first large-scale and explicitly worked-out recognition of the compatibility of the objective and the molar approach is given by Tolman in his "purposive," "molar," or "operational" behaviorism.[74] Since its teleological emphasis on the reaching of "ends" in behavior remains at the empirical level, it is not to be confused with McDougall's vitalistic, "hormic" purposivism (secs. 11, 17, 21). Boring[28] has aptly characterized Tolman's system as the logical positivist-operationist-semanticist revolution within behaviorism.

The most prominent initial foci in Tolman's system are the "cognitive maps" (see sec. 8), generalized spatial orientation schemas represented by the shaded central unit-mark in Figure 7. They are "intervening variables" inferred primarily under the

impact of the realization of the comparative nonspecificity (vicarious functioning) of the particular peripheral-proximal "means" as compared with the relative stability (focal character) of the ends reached. Together with the latter they establish a central-distal reference pattern which sharply contrasts with Watson's earlier peripheralist sensory-motor emphasis. The theoretical shift is the direct result of the unsuccessful attempts in classical behaviorism to find "the" peripheral focus of learning, and of the ensuing experiments, at California and elsewhere, demonstrating the intersubstitutability of kinesthetically different forms of locomotion (such as of running and swimming through a "maze") or of cues in different sense departments.[3, 74] In Figure 7 the new pattern emphasis is represented by the functional arc at right that spans over the oval indicating the motor boundary as a region of vicarious functioning and thus of comparative irrelevance so far as executional detail is concerned. Reference to ends achieved constitutes utilitarian emphasis and thus renders Tolman's system an integral part of objective functional psychology.

It must be stressed that Tolman's intervening variables are not conceived in an introspectionistic manner; that is, they are not taken as criteria indicating the presence of consciousness. They merely take their cue from introspectionism so far as the formative stages of inquiry are concerned; in contrast to classical behaviorism, however, an attempt is made to behavioristically recast the structural elements of our intellectual heritage as congenially as possible (for examples see sec. 12).

In further contrast with classical behaviorism, motivational factors such as "hunger" are actually varied rather than held constant or otherwise taken for granted. They are being operationally anchored in such historical variables as "time since last feeding"; in Figure 7 this is indicated by the oblique arrow (compare also with Fig. 10). Knowledge of the stimulus configuration, of the ends reached, and of the antecedents of motivation constitutes a legitimate three-pronged basis of inference for the intervening variables defining the internal, cognitive-emotional situations. Vicarious functioning enters, at least in principle, into

the necessary scrutiny of the inferences, especially those based on criteria on the response side, by virtue of its incorporation in Tolman's operational redefinition of "purpose" as outlined above (sec. 6; see also sec. 10).

Behaviorism, supported, refined, and broadened by a properly interpreted operationism and methodological positivism, has thus become capable of absorbing the basic molar conceptions of an earlier, vague functionalism and purposivism. To a lesser extent, the resultant system of Tolman has also drawn on gestalt-psychology proper. In one way this has been the case by de-emphasizing, in the theory of learning, the earlier contiguity-associationistic principles of frequent repetition—law of exercise—and of reinforcement by reward—law of effect—in favor of a recourse to figure-like standing out within the perceptual field which (as was first shown by Muenzinger) can be brought about by punishment as well as by reward—law of emphasis. In another way, gestalt influences are apparent in the renunciation, hinted at above, of chain-reflex notions of learning in favor of the "insight"-like transferability of organized segments of spatial patterns. However, the all-important gestalt principle of closure does not loom large in the system of Tolman; molar behaviorism thus remains basically associationistic even though it is generalized rather than specific, selective ("noncontinuous") rather than indiscriminately stimulus-bound association that is being emphasized. On the other hand, molar behaviorism goes beyond gestaltpsychology in extending vicarious functioning from simple, transposition-like instances of vicariousness to a more general exchangeability of "means" and "signs" which would be inconceivable without association; the concept of "sign-Gestalt" used by Tolman in this context has added the semantic depth of distal reference to the mere field orientation of the radical Gestaltists.

Organization in depth is also evident in the concept of "latent learning." It is concerned with the ability to acquire new response-systems in the absence of the specific motivating agent, say, hunger, and with the demonstration of such acquisition by subsequent introduction of an eliciting motivation. Conceptual-

ly, latent learning involves what Carnap[6] has called "dispositional predicates" as exemplified in physics by such concepts as that of 'magnetizeability' (in contradistinction to actual magnetization). Momentarily observable behavioral performance is de-emphasized in a manner similar to Lewin's de-emphasis of the "phenotype" or Freud's de-emphasis of the "manifest."

The multifacet trend in present-day psychology toward a re-vival of emphasis on motivation is also apparent in the introduc-tion of the concept of "operant" behavior, as contrasted with the commonly emphasized "respondent" behavior, by Skinner.[80] It represents an effort to find recognition for the "spontaneity" of the individual in objective psychology.

Brain-and-achievement studies.—A physiological rather than inferential form of molar behaviorism evolves from the experi-ments by Lashley and by others on the influence of brain lesions upon higher learning.[75] The central unit-mark in Figure 7 would have to be replaced by an unshaded one to describe the observa-tional character of the central emphasis. The central physio-logical data are being correlated with the reaching of high-order distal goals such as arrival at the end of a complicated maze.

Along with peripheral variables, some central variables also turn out to be vicarious in that different brain areas are found to be "equipotential." However, the amount of brain area damaged—regardless of its location within wide limits—proves a highly focal central variable by virtue of its being highly correlated with the degree of complexity of a maze an operated animal is capable of mastering.

From intentionalism to thing-constancy research.—The lens model extending left of center in Figure 7 represents the percep-tual constancies discussed in sections 3 and 6. The problem-com-plex involved is known since Helmholtz; the studies in color con-stancy of David Katz, Bühler, and others[9, 76] represent one of the dominant interests of Continental psychology since the 1910's. Thing-constancy research has largely been independent of gestaltpsychology both in personnel and in outlook. It has estab-lished probabilistic central-distal focusing and proximal vicari-ousness in the cognitive domain[77] and thus has gone as much be-

yond nomothetic proximal-central and part-vicarious gestalt-psychology as the latter had gone beyond nomothetic proximal-central but nonvicarious classical psychophysics (cf. Figs. 8 and 9 and the left part of Fig. 7).

Realization of the vicissitudes of probabilistic reconstitution of the object world leads to an emphasis on the degree of achievement, or the utility value, rather than on the technological detail of the thing-constancies. This is in the spirit of American functionalism. Boring[23] has characterized the present writer's emphasis on the "functional use" of perception and the attendant comparative "ignoring of the brain" as an approach which is "less physiological and more biological," in a Darwinian sense, than that of gestaltpsychology. By virtue of the central-distal pattern of reference of constancy research, this approach appears as a symmetrical counterpart to molar behaviorism (Fig. 7), although there is no direct historical relationship between the two.

This symmetry has been elaborated in a joint paper by Tolman and the present writer[78] in which the probabilistic character of molar behavior has also been acknowledged and set in parallelism with that of the perceptual constancies.

Perhaps nowhere is the formal analogy of objective research patterns with, and at the same time their historical descendence from, subjectivistic predecessors clearer than in the field of thing-constancy. Figure 4 contains left of center the encapsulated introspective antecedent to constancy-research, given by the intentionalisms of Brentano, Meinong, and James (sec. 16), to which Figure 7 is like the unfolding of a seed. It is perhaps for this pictorial analogy that Hilgard[3] has recently labeled the present writer's outlook upon thing-constancy as a subvariety of "act psychology." However, while Brentano's "intentional objects" had to be classified above as derived from introspection and not checked by measurement, the "attained objects"[77] of thing-constancy are independently constituted stimulus variables in the sense of physicists' constructs which have been found statistically to correlate with the responses. It is in this manner that the old introspective-metaphysical question as to whether we "see" the retina or the outside world can be given an operational meaning which is testable, in terms of "achievement," by functional validity coefficients.

"Psychology in terms of objects," "psychology of the empty organism," mental testing.—Determination of stimulus-response correspondences easily leads to a shift of emphasis from the re-

sponses to the stimuli correlated with them. Suggestions in this direction, although with an accent on proximal more than on distal stimuli, have been made by Bekhterev[70] and, somewhat less unequivocally, also by Uexküll.[79] The present writer[77] has spoken of a "psychology in terms of objects" (*Psychologie vom Gegenstand her*) in which organisms are described, and differentiated from one another, by reference to the—predominantly distal—stimulus or result variables with which they have "attained" stabilized relationships. By applying this approach to distal-to-distal functional arcs bridging over the entire organism without descending into it (Fig. 7), one may further gain in scope and at the same time get around the hazardous construction of intervening variables. Such an approach may be characterized as "without, yet about, the organism"[1] in that it is concerned with relationships established by the organism although not with their organismic anchorage. In basic conception, this approach is closely related to what Boring[35] has called "psychology of the empty organism," in referring to Skinner's[80] proposal that "psychologists had better give up the nervous system and confine their attention to the end-terms." In specifying this proposal by urging positive ascertainment of focusing, and of the width of vicarious functioning in the proximal versus the distal region(s), we can avoid focal-arc atomism (sec. 8) in spite of the ignoring of intra-organismic mediation. The full-fledged pattern of functionalistic research can be realized in this manner, thus removing what must seem the most cogent basis for criticism of the empty-organism approach (see also sec. 10 on economics vs. technology).

Whenever psychological research dispenses with mediational pillars within the organism, the subject must be defined indirectly or in terms of a more or less broadly conceived class. In animal experimentation, one may refer to species. In mental testing—perhaps the most prominent illustration of the empty-organism approach—one may specify individuals or populations in terms of such generic concepts as nationality, chronological age, normalcy, or socioeconomic status. The crucial point is that this kind of research is conceived of without reference to, or scrutiny

of, organismic detail or the particulars of the actual execution of the test. On the other hand, the test problems themselves are precisely specified, and so are the solutions that have issued from the individuals—and that define the test-"scores." All this is in terms of objects attained, much in the way one might characterize the over-all behavior of cats by pointing out that mice (distal stimulus) disappear (distal result) in their presence.

Neo-facultative factor analysis.—In the mathematical analysis of test results thus obtained, "factors" may be extracted through statistical intercorrelation of the responses of large samples of individuals. Some psychological statisticians prefer to remain strictly within the distal region by considering the factors merely as self-contained abstractive "condensations" or mathematical artifacts without inferential implication. Others, notably Thurstone, tend to see the factors as intervening variables or, more specifically, as hypothetical constructs. In this sense, factors would represent "functional unities" within the organism and serve as operationally defined successors to the "faculties" which had become disreputable in the wake of speculative psychology.[81]

One of the objections to be made to faculty psychology from the point of view of the philosophy of science, to the effect that it constitutes an unnecessary (metaphysical) duplication of observed behavior, can be met by pointing to the many-many rather than one-one character of the relations involved; in the general case, each factor emerges as the result of a multitude of underlying test performances, and vice versa. The suggested regional separation of the dispositional or covert from the manifest or overt traits then appears justified in the light of the principle of vicarious functioning; two individuals with identical scores on a certain test may have produced them by quite different methods or patterns of factor-endowment, and it is only through combination with further tests that some degree of univocality is restored to the appraisal of underlying factors in the individuals.

One of the chief difficulties in the consideration of factors as dispositional "realities" lies in the fact that from a purely mathematical point of view a body of data can be factored in many different ways. Thus Spearman's original assumption of a "general factor," g, which is common to (nearly) all intellectual

activities, is in a formal sense compatible with Thurstone's multiple-factor theory which postulates a minimum overlap of common factors responsible for the performances in the various tests. A resolution of this dilemma cannot be achieved without invoking the principle of parsimony (see sec. 11); this is actually done by Thurstone in his method of "simple structure" which curtails the possibility of arbitrary "rotation" of the axes representing the factors. Thurstone considers the fact that the "primary" factors obtained by this method have reappeared in successive studies with different test batteries as an empirical confirmation of their "psychological uniqueness." He expresses the hope that "mentality is, in this sense, not formless, but that it is structured somehow into constellations of processes which will eventually be identified."

Thurstone is conversant with the fact that, by proceeding from the computation to the "interpretation" of factors, he may transcend "objectivity" of construction. There are two major problems of interpretation. One is the physiological, genetic, endocrinological, sociological, or other independent identification (see sec. 10) of the factors in terms of what Tryon has called "radical components." The second problem of interpretation—more delicate from the standpoint of objectivistic language than the first—is that of the "naming," or "psychological meaning," of the factors. In this respect factors seem to be somewhat like the abstract, "nonpictorial" constructs in modern physical theory (see Philipp Frank, this *Encyclopedia*, Vol. I, No. 7). However, while in physics the difficulty usually lies with "visualization" of the construct in a more or less distinctly spatial sense, in the case of factor analysis it is more the introspective empathy into the factors or the possibility of their identification with common mental or behavioral operations which is deficient.

As in physics, this is no objection to the usefulness of such constructs. Thurstone prefers naming his factors by "well-known concepts"—such as facility with numbers, or self-restraint in making rational inferences—so as to render them "psychologically challenging," rather than just labeling them by a series of letters. And whether or not, say, the number factor may have to be characterized more broadly as "facility with a certain type of chain association" (as was suggested by Landahl) will, according to

Thurstone, eventually resolve into an empirical check concerning its nature by means of further tests.

21. Dynamic Personality Theory

In this section we will deal with convergence products of centralism, functionalism, and psychoanalysis, mostly characterized by a relatively low level of formalization.

Dynamic psychologies, cognitive and motivational.—Various psychologies with centralist leanings have, at one time or another, resorted to designating themselves as "dynamic." In the case of Köhler[69] the term refers to central-physiological "field"-interaction as it plays a part in cognitive organization (sec. 18). Developments toward a "dynamic psychology of cognition" have been surveyed by Heidbreder,[82] who compares her own studies on the "attainment of concepts" with Goldstein's emphasis on abstraction and the approaches by Woodworth (see below) and by the present writer (Chap. II and sec. 20). More recently the term 'dynamic' has acquired a more motivational rather than cognitive connotation. This is the case with Lewin's "vectors" and "valences" (sec. 22), which deal with relatively transient motivational states; the theory of "personality dynamics," to be discussed in a moment, deals with more enduring emotional and motivational patterns conceived under the direct or indirect influence of psychoanalysis (sec. 16) with its elaboration of the core of the "person" which is left relatively undifferentiated in the system of Lewin.

An earlier variant of a motivationally oriented "dynamic psychology" is that of Woodworth,[83] one in Heidbreder's list of seven major psychological systems.[50] It stresses the need to expand the peripheral type of stimulus-response or S-R psychology into an S-O-R and eventually into a W-S-Ow-R-W psychology, whereby 'O' stands for the organism including the drives, 'W' for the world, and the 'w' attached to O for the organism's "situation-and-goal set." Woodworth's complete scheme thus anticipates the fivefold sequence from distal stimuli to distal results as presented at the upper right of our main table of figures and is thus close to modern functionalism. Like Tolman and most other modern functionalists, Woodworth believes that the science of psychology has little to do with the question of mechanistic reducibility of motivational factors. By the same token, however, macro-mediation is not given sufficient attention. In consequence, there is no proper recogni-

tion of vicarious functioning and of the comparative irrelevance of the peripheral stimulus and response regions, so that in reality the system shrinks to what Woodworth discusses as a W-O-W pattern; it remains too sketchy for the actual execution of a full-fledged W-(S)-O(R)-W scheme, as the molar behaviorism and probabilistic functionalism referred to in section 20 and in Figure 7 might be described in Woodworth's notation.

Need-effect psychology. Experimental and statistical approaches to personality dynamics.—The instinct theory and some aspects of the procedure of psychoanalysis including the principle of "projective" interpretation of manifest data have exerted considerable influence upon the theoretical and clinical psychology of personality, either directly or through Murray and his Harvard collaborators.[84] Murray has properly exposed the comparative irrelevancy of the mediating peripheral-proximal "actones" (see Fig. 10); yet his approach is not free of unscrutinized inferences regarding the hypothetical central "needs" on the basis of distal behavioral "effects." In spite of occasional warnings to the contrary on the part of Murray himself, his system is dominated by a kind of tacit constancy hypothesis regarding the coordination of the need and the effect region albeit with a direction of inference opposite to that of psychoanalysis (see sec. 16). As was argued by Frenkel-Brunswik,[62] the "casting of a duplicate copy . . . of a certain overt and essentially distal pattern . . . into the central region, by just adding . . . the phrase 'need for' . . . is in fact not central, but distal psychology." She has therefore attempted to establish an "independent" approach to the central and has statistically studied the "and/or" families of phenotypically sometimes quite unrelated "alternative manifestations" of the same need in overt behavior; operational justification can thus be lent to the concept of motivation in accordance with patterns of vicariousness first developed in psychoanalysis.[85]

Numerous efforts are being made in dynamic psychology[86] to test a variety of further psychoanalytic hypotheses by objective methods.[87] Evidence, when collected with a proper understanding for the underlying psychoanalytic assumptions, is on the whole encouraging. For such topics as repression the technique of unfinished tasks developed in the school of Lewin has proved

useful; the importance of early infancy in the establishment of the adult personality has been verified in experiments on animals. There also was direct experimental confirmation of the crucial claims concerning symbolism.

Situational-cultural approach.—The original historic-genetic emphasis of psychoanalysis on "infantile phantasies carried on into adult life (for) imaginary gratifications"[63] has recently given way not only to a more functionalistic ego-psychology (sec. 16) but also to a more "situational" emphasis in the tracing of causes that affect the central picture (see Fig. 10). This development, although to a certain extent represented by Murray's emphasis on external "press" on the individual, finds its major expression outside of psychology proper, in Fromm and in Horney, as well as in a group of cultural anthropologists, notably Kardiner.[88]

Recognition of the importance, in establishing basic personality patterns, of the impact of maternal care, sibling interactions, paternal dominance, and of conduct more indirectly related to social institutions perpetuates psychoanalytic emphasis on infancy and early childhood. But specific tracing of how the earliest components of action systems were learned and integrated injects an element of Pavlovian thinking. The emphasis on "reality systems" which take care of dealings with the outer world and with other people forms a counterpart to the "projective systems" in religion, folklore, and the arts and establishes contact with the ego-psychology of the orthodox modern psychoanalysts.

22. Nomothetic Encapsulation in Topological Psychology, Postulational Behavioristics, Mathematical Biophysics

Discussion in this section concentrates on what is commonly known as psychological "theories" in the narrower, formal sense of the term. Nomothetic rigor is, so far as the actual body of the theory is concerned, in each case attained at the price of withdrawal within boundaries too narrow for adequate scope. Confinement is within the central region in the case of Lewin, and within a peripheral-central system in the case of Hull.

Encapsulated centralism in topological-dynamic psychology.—Gestaltpsychological, molar behavioristic, and formal-nomothetic features are combined in the theories of Lewin.[89] In his "topological" psychology he attempts graphic representation

of the cognitive and motivational central time-slice which he calls the "life space" or "psychological environment." This is in preparation for a "dynamic" analysis in terms of hypothetical directional forces or "vectors" which leads to the development of laws allowing prediction of the future states of the system. Here we are to study "systematic" (or nomothetic) causation in terms of generalized essential conditions rather than "historical" (or idiographic) causation which traces individual occurrences through their actual, more incidental past.

The theory contains reference neither to the objective stimulus conditions nor to the motor execution and the further results of a response. The system and its laws thus remain confined to a postperceptual, yet prebehavioral, "contemporaneous field" in the central area (see Lewin[1] and Brunswik[1]). This field possesses a solipsistic or monad-like if intentionalistic character (see sec. 16). For this reason, Lewin's topological diagrams correspond to the dotted oval representing the theater of reference of intentionalism in Figure 4, especially to its right-hand side describing directedness toward a "goal" not yet attained. The organism itself appears in the life-space by means of its relatively undifferentiated central reflection, the "psychological person." The goals have different "valences," and the path to them is more or less "segmented" by "barriers" into gestalt-like unitary "regions" (the term used differently than in this monograph, where it refers to objective organism-environment stratification) which are anticipated to lie between person and goal. Lewin's shying-away from the peripheral and "phenotypical" in favor of the underlying "genotypical" central events has induced Hull[1] to concur with the present writer in his criticism of Lewin's system as an "encapsulated" one.

Programmatically Lewin's drawings are meant to refer to an objective constellation of central forces within the organism, rather than to a primarily conscious reality as is the case with intentionalism proper (act-psychology). However, Lewin's actual source of verification of the life-space is one of the following three token ones: (1) introspective phenomenology, as critically claimed by Spence;[1] (2) causal one-to-one response-inference without scrutiny in terms of vicarious functioning, in that occurrence of the first or the most prominent member of the respective habit-

family-hierarchy. with its potential or incidental reaching of the distal end is taken as a criterion (this procedure, also hinted at by Spence, was more explicitly criticized in sections 6 and 11); (3) surreptitious one-to-one inference from the stimulus. The last-named fallacy is exposed by one of the most frequently heard objections against Lewin, accusing him of *de facto* confusion of geographical with behavioral (life-space) environment in spite of lip service to their conceptual separation.

In fairness to Lewin as an experimentalist, it must be added that he and his collaborators have extensively studied "substitution" of tasks and other aspects of vicarious functioning.[88] Perhaps the clearest expression of Lewin's *theoretical* detachment from the distal aspects of behavior is given by his explicit repudiation of the study of achievement. He has declared the question as to whether or not an animal actually reaches its goal to be psychologically irrelevant (see Brunswik[1]). In stating that "one will have to give up, in principle, to organize processes defined merely in terms of 'achievement' (*Leistung*) under unitary, psychological laws,"[89] Lewin in effect subscribes to our considerations of sections 7 and 9; the difference is that we do not consider the impossibility of a strictly nomothetic treatment of achievement sufficient reason for the excommunication of this aspect from psychology.

While Lewin in effect states that after having *conceived* of a certain situation the individual *will want* to do such and such, the molar behaviorist or probabilistic functionalist in effect assumes that when *placed* in a certain situation the individual *is likely to arrive* at such and such end (which will be part of the criterion of his "wanting" to do so). The former type of intra-systemic prediction, considerably narrower in scope than the latter, may possibly be made with nomothetic certainty; the latter, encompassing as it does the intricate problem of mastery of the causal texture of the environment on the part of the organism, is bound to remain probabilistic.

It is one of the assets of the system of Lewin that quantification of the life-space is rejected, and Euclidean, metric space replaced by the non-quantitative yet mathematical device of topological space based on relations of order rather than on measurement proper (see sec. 11). This is well in keeping with the phenomenological interpretation of Lewin's life-space suggested by Spence. In fact, regardless of the somewhat disputed mathematical soundness of topological psychology, topological representation may be the long-sought answer to the introspectionist dream of finding a mathematical, and thus univocal, "objective" means of communication for data of consciousness in spite of their intrinsic—absolute or relative—non-quantifiability. In granting this, one should not forget that objective language does not in itself guarantee objectivity of the observations which are to be communicated and that no result can be more objective than the most subjective of its single steps of observation, inference, or communication (see sec. 3).

Peripheral-central encapsulation in formalized learning theory.—While Watson may be said to have succumbed to all the biases of objectivity listed in Chapter III, and Tolman to have avoided them all, the founder of a third major movement within the framework of behaviorism, Hull, has successfully overcome hostility to theory and to central constructs but in turn displays all the characteristics of emulative physicalism.

In the first of two books,[89] published in 1940 in collaboration with members of his group at Yale, an effort is made to develop a "hypothetico-deductive" theory of so-called mechanical or "rote" learning in attempted analogy to the axiomatic method in geometry. Under the stimulus of Woodger (see this *Encyclopedia*, Vol. II, No. 5) symbolic logic is used in addition to more conventional mathematical derivation with the intent of assuring utmost "clarity" in the sense of objective rigor (sec. 4).

An effort is made to embrace both the dynamic factors assumed by Ebbinghaus for rote memory and those of Pavlov for conditioned reflexes within a unified system based on "postulates"; these are to be expanded and recast in accordance with new evidence collected with the express purpose of experimentally checking or rechecking the theory. The principle of habit-formation through reinforcement by contiguous need-reduction (effect) is stated in the fourth postulate. Definition of such "unobservables" as "stimulus trace," "excitatory potential," and "inhibitory potential" is seen as a parallel to that of such physical constructs as gravity, energy, or atom. The list of eighteen stated postulates is set in analogy to that of the thirty-nine estimated by Malisoff to underlie mathematical physics.

In a second book, more encompassing and more directly concerned with "principles of behavior,"[90] Hull further proceeds to reconcile the learning principles of Pavlov with those of Thorndike, especially the law of effect (sec. 18). This as well as extensive use of the utilitarian concept of the 'survival' of self-maintaining organisms characterizes the system as functionalistic at least in intent. But very few of the experiments cited are concerned with higher, genuinely adjustive types of learning problems. In any event, the system stays within the framework of associationistic tradition; there is token recognition of stimulus patterning and other gestalt principles, primarily through such

relatively casually superimposed principles as that of "afferent neural interaction" and "behavior oscillation." As Skinner has pointed out in his critical review,[90] these additions lessen the rigor of the system by blurring "the concrete manifestation of empirical laws."

Hull shares with Tolman the inclusion of motivational factors, such as needs and drives, into the behaviorist system. He also has adopted Tolman's terms 'molar' and 'intervening variable.'[1] In general, the conceptions and inferential procedures of the two authors resemble each other in many respects more closely than appears on the surface; this has also been recognized by Bergmann and Spence[1] and by Spence[1] along with the fact that Hull's method is not axiomatic in the strict sense of the term but merely a gradual adjustment of tentative hypotheses. However, while Tolman's definition of purposive behavior lays stress on vicariously mediated distal results in the constitution and inferential ascertainment of intervening variables (see sec. 6), Hull's major concern is with the interrelationships of these intervening variables among one another and with peripheral-proximal boundary conditions as they may be formulated in a system of laws. Hull's theory is entirely contained within the limits of the organism, as can best be seen from the concluding diagram of his *Principles*, depicting his entire system. The adjustiveness of learning, as reflected in such concepts as "consummatory response," finds representation only in its intra-organismic reflections, that is, with the central end of the feedback loop rather than with the loop in its distal, probabilistic portions. He is thus paying with molecular encapsulation for progress along nomothetic lines (concerning some basic confusions in Hull's nomotheticism see sec. 11). The entire technique of stressing, in the establishment of central variables, the customary type of univocal equations is an indication of the limitations of the system. The importance of adaptive probabilities is only vaguely acknowledged in the last few pages preceding the summary of his *Principles*.

The neglect of the principle of vicarious functioning becomes especially obvious in Hull's diagrams used in discussing inter-

vening variables.[1] He seems to take for granted the legitimacy of inferring these variables from proximal stimuli or responses in a simple one-track or parallel-track manner in the way of a constancy hypothesis (see sec. 2). This methodological naïveté, and the neglect of well-focused distal reference inherent in this naïveté, not only holds for Hull's own system but is also present in his attempted representation of Tolman's and of the present writer's procedures along with those of Carnap.

In consequence of all this, Hull has, in his formalized writings, lost sight of what in the opinion of the present writer is his most significant contribution to psychological theory, namely, his concept of "habit-family-hierarchy" (sec. 6). As was pointed out by Ritchie,[90] Hull's present analysis is concerned with segments of behavior too small to require the introduction of such truly molar concepts. As temporarily during the first wave of behaviorist enthusiasm, there is a loss in level of complexity in the wake of a gain in rigor (see secs. 18 and 19).

Many of Hull's theoretical constructs are labeled in terms remindful of physiology. Examples are 'neural interaction,' 'efferent impulse,' 'reaction potential.' As has been pointed out by Spence,[90] however, the surplus meaning accruing by implicit or explicit allusion to a hypothetical neural identification may add little to the theory.

Somewhat akin to one of Hull's own efforts is an attempt by Woodrow[91] to find a single "generalized quantitative law" covering a wide variety of topics. In this case the suggested principle extends, among others, to learning, psychophysics (presenting an alternative to the Weber-Fechner law), and the development of intelligence. It combines two hypothetical factors, one for the physiological ceiling inherent in all performance, the other representing what might crudely be labeled "sensitivity." Woodrow thus follows the example of Hull and of Rashevsky (see below) in proceeding from an abstractive synopsis of the curve-fitting type to assumptions concerning underlying dynamics. The formula itself is an exponential function with a large number of parameters. Since, contrary to the "universal" physical constants, such as the speed of light, Woodrow's parameters vary from one context to another, the formula can be fitted to an extremely wide range of curve shapes, thus reducing the value of synopsis which would seem to lie in comparative specificity.

An alternative procedure, more in keeping with the tenets of this monograph, is that of Estes,[92] who attempts, for a variety of problems in the domain of learning, to "systematize well established relationships at a

peripheral, statistical level of analysis. . . . Likelihood of responding [is] taken as the primary dependent variable; . . . independent variables are statistical distributions of environmental events. Laws of the theory state probability relations between momentary changes in behavioral and environmental variables."

An example of approaches in which physiological orientation becomes dominant is given by some recent attempts to synthetize psychology about the concept of "homeostasis" as developed by a general manner by Cannon and as increasingly applied to explanatory problems in psychology, psychopathology, and the psychoanalytic theory of instincts.[93]

Rashevsky's quasi-neurological mathematical biophysics.—Extensive use of quasi-neurological models including hypothetical hierarchies of brain centers is made in the type of mathematical biophysics developed by Rashevsky with his collaborators at the University of Chicago and by others.[94] It is broader in psychological scope than that of Hull yet less well known among psychologists. Starting from a simplified schema of cellular occurrences, it has succeeded in mathematically deducing experimental results in the field of higher mental functions with considerable specificity and accuracy. This holds for problems of thinking, gestaltpsychology (especially ambiguity and the transposition of the "universals" of form), perceptual thing-constancies, aesthetics, equipotentiality of brain areas, along with elementary sensory discrimination and conditioning. More recently the theory has also been extended to social psychology, social ecology, and sociology in general.

The variety of possible and not necessarily incompatible theoretical approaches to one and the same psychological problem is well illustrated by the efforts to explain the aesthetic appreciation of geometric forms. Birkhoff[95] had postulated, in a somewhat intuitive way, that certain geometric characteristics possess a certain arbitrary degree of pleasantness or unpleasantness. A different approach is made by the mathematical biologist. He begins with a set of assumptions about the mechanism of the central nervous system and tries to specify the processes that correspond to pleasantness or unpleasantness. Rashevsky assumes that four hypothetical brain centers take care, respectively, of the stimulus factors of (1) verticality, (2) horizontality, (3) angularity, and (4) length, and he arrives at predictions well in agreement with experimental results. However, an independent factor analysis of the stimulus bases for judging Birkhoff's figures[96] leads to the isolation of four quite differently conceived factors.

These are (1) smoothness (as contrasted with "blockishness" which is disliked)—possibly also to be interpreted as "complexity of design within a simple outline"—(2) simplicity in a geometric-conventional sense, (3) symmetry (especially rotational and diagonal and only secondarily horizontal and vertical symmetry), and (4) odd points or upward-reaching tallness. The discrepancy between the two approaches raises once more the problem of the "reality" of factorial constructs (see sec. 20).

McCulloch and Pitts[94] have elaborated on structural similarities between logic and nervous activity, thus bridging over to the mathematical theory of communication. In this latter approach (to be discussed in sec. 23), each "freedom of choice" defines a unit or "bit" of information, the term condensed from the binary digits, 0 and 1, which are used to replace the ten-digit series of customary arithmetic. Since firing in nerve elements follows an all-or-none principle and may thus be idealized as dichotomous, McCulloch[97] has pointed out that "neural events and relations among them can be treated by means of (two-valued) propositional logic" in a strictly nomothetic manner and that all psychic events have an inherently "intentional," "semiotic," or "purposive" character.

In their earlier paper the authors further point to the presence of feedback circles (see sec. 23) and stress that the inclusion of disjunctive relations leads to uncertainties in the disentanglement of causal relations which prevents complete determination of preceding states of the world or of ourselves. "Even if the net be known, though we may predict future from present activities, we can deduce neither afferent from central, nor central from efferent . . . activities." The asserted "ignorance implicit in all our brains" would seem to have significant implications not only as to the necessarily probabilistic character of perceptual and behavioral extrapolations on the part of the organism but also as to the risks involved in the use of "response-inferred" constructs in psychological theory (see sec. 10).

Micro-probabilistic approach in the analysis of neural nets and of social interaction.—The analysis of McCulloch and Pitts just referred to has been taken up by Rashevsky as a microscopic underpinning to the fundamental equations of mathematical biophysics much in the manner in which the microscopic kinetic

theory of gases serves as a reductive support to the relatively macroscopic thermodynamics with its more direct access to empirical verification (see sec. 9). Together with Landahl, the first-named authors have undertaken to develop a formal method for converting logical relations among the actions of neurones in a net into statistical relations among the frequencies of their impulses.[94] The correspondence is likened to that of Boole between the algebra of logic and that of probability.

In this manner the earlier biophysical study of the average effect of large numbers of hypothetical neurones was paralleled by a study of "random nets" whose structure is "not a clearly defined topological manifold such as could be used to describe a circuit with explicitly given connections, since concern is not with 'this' neurone synapsing on 'that' one, but rather with 'gross' distribution of tendencies with points or regions in the net." Starting from net-tendencies, probabilistic dynamics based on fundamental assumptions about the behavior of individual neurones (such as threshold, synaptic delay) are applied to derive the probable characteristics of behavior (or order out of chaos in the sense of Wiener; see sec. 23). This statistical "rather than deterministic" biophysics was developed by Anatol Rapoport and by Shimbel.[94] It has in the main retained its basically microstatistical character even in its subsequent extension to the dynamics of social interaction in the sense that the underlying individual occurrences do not attain observational identity (see sec. 9). The approach must thus not be confused with the macro-probabilistic concern with individual probability predictions in cybernetic communication theory (sec. 23).

23. Brain Models and Statistical Extrapolations in Cybernetics and Communication Theory

Negative feedback circles in servomechanisms.—Of great importance to psychological theory are the considerations developed in the course of the construction of computing machines and related mechanical and electrical "brains" during the second World War. The approach has been labeled "cybernetics" by

Wiener,[97] after the centrifugal "governor" which establishes automatic self-control of the intake of steam into an engine. One of the basic principles involved in such "servomechanisms" is known in engineering as "feedback." Feedback occurs when part of the effects of a given process are channeled into a "circular" loop so as to revert upon the source of the process in question. In the case of the steam engine and of all healthy biological self-compensation the feedback is inverse or "negative," that is, it neutralizes excessive conditions so as to establish a periodic-stationary stabilization or "steady state." Writing on what Charles Bell had called the "nervous circle," the mid-nineteenth-century French neurophysiologists defined the "reflex" as an activity which, originated by a change in some part of the body, proceeded to the central nervous system, whence it was reflected to that part of the body where it had been initiated, and there diminished, stopped, or reversed the change that had given rise to it.

The psychological reader will easily recognize the similarity between the idea of feedback and the pivotal concept of American functionalism (sec. 16), that of adjustment through the setting of causes which in their aftereffects reflect upon the organism as correctives of its initial conditions. But while in psychological functionalism the circular loop is apt to include smaller or larger portions of the surroundings before it boomerangs—mostly in a beneficial though sometimes in a harmful manner—the feedback loops of computing machines are contained within the system itself. In classifying the "teleological mechanisms" described in cybernetics at large, McCulloch[97] distinguishes "appetitive" negative feedback devices, in which the "circuit passes through regions external to the system," from such simpler cases of feedback as the "merely homeostatic" which keep some internal parameters of the system constant, or the "servo," in which the value of the parameter or end sought by the system can be altered from without the circuit (whereby they may become instruments of superordinate systems). The self-contained case corresponds to the peripheral-central model with encapsulation within the organism discussed in section 22. It easily falls into

the simplified pattern of a nomothetic single-path ideology; this is, to be sure, entirely adequate in the case of computational tasks involving merely the relations between "input" (peripheral stimulus) and "output" (peripheral response) without venturing beyond the boundary of the system proper.[98] Significantly, Wiener's book *Cybernetics* bears the subtitle "control and communication *in* the animal and the machine" (italics ours).

Both mathematical biophysics and cybernetics have claimed to be able to cover, with their machine-like conceptions, the internal dynamics of gestalt processes. On the other hand, Bertalanffy[59] has argued that "feedbacks, in man-made machines as well as in organisms, are based upon structural arrangements. Such mechanisms are present in the adult organism, and are responsible for homeostasis. However, the primary regulability, as manifested, for example, in embryonic regulations, and also in the nervous system after injuries, etc., is based upon direct dynamic interactions."

Macro-statistical extrapolation of extrasystemic sequences in appetitive mechanisms.—It is of utmost significance that in a number of specific instances cyberneticists have transgressed the limits of the machine boundary and have demonstrated the suitability of their conceptions in dealing with problems in which the spatial or temporal environment is made part of a larger system. In "appetitive" closed paths (see above) the machine or the organism is no more than a link, and predictive problems as to the events to be expected in the surroundings come to the forefront. It is primarily in such extended contexts that cybernetics becomes involved in a "recasting and unifying of the theories of control and communication . . . on a statistical basis" (Wiener[97]).

While in principle everything may be under strict control within the machine, the remote space-time surroundings are in the general case known to the system by extrapolation only, that is, predicted with some uncertainty. As psychological functionalism, when actually carried out, has thus been found to be forced into probabilism, a cybernetics with ecological involvement must contain probabilistic elements. A guided missile can at best compute the *probable* future positions of the airplane it is chasing, but at the same time it must do so in order to be efficient.

For guns to shoot at the place where the target would (probably) be when the shell arrived, analysis of past observational data becomes necessary. This means introducing the macro-statistical approach we have tried to point up as essential for the objective study of organism-environment relationships in behavior (sec. 9). And it also means incorporation of "memory" or of "learning" from past success or failure into the devices under discussion, which may go so far as continuously to modify behavior as results appear.

The extrapolations into the future are based on the "stochastic" character of the sequences involved, that is, the presence of statistical regularities which, when the length of the sample is reasonably large, will allow probability-prediction provided that the over-all character of the process remains fairly stable. Wiener has demonstrated that there is an "optimal" prediction based on past experience of the system. He goes on to explain that the autocorrelations and intercorrelations of data in time series used in such analyses may detect the existence of causal connections, and their lags in one or both directions, provided the correlation is less than perfect. In this manner we may also detect feedback, including inverse feedback, in ecology, anthropology, and sociology. The data need be no more than decisions, actions, or opinions in time, provided we have runs of sufficient length. Man should learn to recognize and to decrease the gain in those reverberating circuits that build up to open aggression. By means of long-time runs of data we can determine whether the mechanism of negative feedback accounts for the "stability and purposive aspects" of the behavior of groups.

A first step in the direction of such bivariate correlation is made by Wiener in his analysis of multiple time series and, in particular, double time series:

"Such series occasionally make their appearance in engineering applications of the theory, but they are most conspicuous in the statistical applications, both economico-sociological and meteorologico-geophysical, since in both instances the relative lead of one time series with respect to another may well give much more information concerning the past of the second than of its own. For example, on account of the general eastward movement of the weather, Chicago weather may well be more important in the forecasting of Boston weather than Boston weather itself."

It may be noted that even here the two variables (or groups of variables) used in the example are of the same kind or physical dimension(s) or denomination(s) ("weather"); in such cases

comparisons or correlations are not yet as genuinely bivariate (or multiple) as are those between psychological cues and objects or means and goal-attainments.

Ancillary to the emphasis on unidimensional sequential distribution predominant in cybernetics is a pioneer study on "statistical behavioristics" by Miller and Frick.[99] Their actual exposition encapsulates within the single region of overt responses. Shannon's sequential statistics as exemplified by the study of characteristic stochastic word-sequences in common English are applied to an analysis of the "courses of action"—related to the "strategies" in the playing of games[100]—and the "dependent probabilities" resulting from the fact that the preceding occurrence of a response does not always return the system to the original state. Following the suggestion by the early behaviorist, Hunter, that a response influenced by the penultimate of the preceding responses indicates a symbolic process, it is made quantitatively palpable by means of these data that seven-year-old children have a greater ability for symbolic processes than do rats, while the latter also possess that ability to a certain extent. An "index of stereotypy" of behavior used in this process may become useful in the application of Tolman's or Krech's objective criteria of purposiveness or of the presence of "hypotheses" (see sec. 12).

An example of the fact that not only bivariate correlation but even the basic schema of vicarious functioning has entered the scope of cybernetically oriented work outside psychology proper (albeit on a relatively nontechnical level) is given by a paper comparing various alternative sets of flight instruments in insects and in modern aircraft.[101] The issue concerned in this example is, to be sure, primarily that of the varying availability of cue systems rather than the more intrinsic and complex statistical one of the dependability of a certain type of cue if and when it is available.

As to this dependability of cues, the fact that air movement past the insect can be used to simulate its normal passage when flying, and will in turn lead to the characteristic stereotyped response pattern, may be taken as a drastic and somewhat atypical analogue to the possibilities of deception wherever input (or cue)—say, air pressure—is uncritically taken as a univocal representant of its presumed distal referent—say, air speed. The danger of being misled in prediction could be taken care of only in a probabilistic manner by statistical correlation so long as consid-

eration remains confined to the limited set of variables available to the particular mechanism or subsystem concerned.

Thinking, perception, telecommunication.—To take for granted univocal interrelations among different types of events is to a certain extent justified so long as one has in mind only the psychological texture typical of "thinking" in the sense of explicit logical reasoning. It ceases to be adequate when we include the less ideally executed patterns of thought or the compromise type of probabilistic quasi-reasoning on the basis of insufficient evidence which is implicit in the more primitive form of cognition usually called "perception" (see sec. 7). Ideal rational thinking manages to isolate cues of highest dependability and thus is enabled to switch over vicarious to single-track functioning with not only no loss but even a gain in univocality. This lends a narrowly machine-like quality to discursive thinking; it is precisely this quality which is represented in the "machines that think" of cybernetics and in some of the concepts of mathematical biophysics (see sec. 22).

In airplanes or guided missiles which continue to "think" while flying or seeking out their target the mediation processes may well in first approximation be considered as patterned in a similarly idealized manner. The "posits" of perception and of overt behavior, on the other hand, are in some cases more like ordinary passive missiles intrusted to a highly erratic medium at a stage far removed from the goal and imbued with considerable uncertainty as to reaching it. In communication engineering, the closest analogue to this is the distortion of telephone messages and related problems of distant communication. A more generalized exposition of those of the mathematical principles of communication which are of particular relevance to the understanding of vicarious functioning in psychological mechanisms has been given by Shannon and by Weaver.[102]

Using the vocabulary of the special brand of telecommunication engineering from which the theory has taken its start, perceptual cues and behavioral means are like "signals" in "coded messages." The perceived objects and behavioral results which correspond to the message are mediated through "noisy channels." These latter are contaminated with interferences or con-

straints of their own which reduce the sender's freedom of choice (also called lack of bias, degree of randomness, or "entropy") in the medium that must be molded to carry the message. The result is "equivocation" (or "entropy of the message relative to the signal"). It is then "not in general possible to reconstruct the message with certainty by any operation on the signal."

The inherent tangledness of the causal texture of the environment of a behaving organism may be seen as a specific type of "noise." We are reminded of Heider's[20] juxtaposition of the ideal "medium," such as the electromagnetic field, which is pliable to one kind of influence with a minimum of interference, with "things" which have firm properties of their own. These latter can be imposed upon the medium, thus becoming messages in the sense of communication theory. In all this, the undesirable uncertainty arising from structural or statistical properties of the medium is in inverse relationship to the desirable uncertainty which arises by virtue of freedom of choice of the message to be transmitted.

Shannon's diagram showing the fanning-out of "reasonable causes" (messages, inputs) for a given "high probability received signal" or effect, and of "reasonable effects" (signals, outputs) from a given "high probability message" or cause "in a channel," bears formal resemblance to the present writer's diagram showing the univocal and equivocal types of "coupling between intra- and extraorganismic regions"[77] which can also be read into our diagram of the lens model (Fig. 1).

If the richness of variability, or the "capacity," of a channel is less than the richness of variability, or the "entropy," of the source from which it accepts messages, then it is "impossible to devise codes which reduce the error frequency as low as one may please.... However clever one is with the coding process, it will always be true that after the signal is received there remains some undesirable (noise) uncertainty about what the message was." The channel then is "overloaded."

We may add that the crux of organismic adjustment obviously lies in the fact that distal perceptual and behavioral mediation must by the nature of things in the general case rely on overloaded channels, with the ensuing limited dependability.

Redundancy as an antidote to equivocation.—However, the authors[102] point to one means by which the chances of error can be decreased. This is "redundancy," as exemplified by repetition. The English language is about 50 per cent redundant, by virtue of grammatical and other structural properties which make certain sequences of letters or words more likely than others. It would be possible to save about one-half the time of telegraphy by a proper encoding process, provided one was going to transmit over a noiseless channel. When there is noise on a channel, however, there is some real advantage in not using a coding process that eliminates all the redundancy. For the remaining redundancy helps combat the uncertainty of transmission.

We may add that vicariousness of psychological cues and means may be viewed as a special case of receiving or sending messages through redundant, repetitive channels, thus reducing the probability of errors, that is, the set of possible causes, or effects, that could result in, or be produced by, the type of event in question. Vicarious functioning is thus indeed of the essence of behavior.

Relevant to our above discussion of an "objective language" in science and its close relationship to statistical reliability and validity (secs. 4 and 7) is the following quotation from Weaver:[102] "Language must be designed (or developed) with a view to the totality of things that man may wish to say; but not being able to accomplish everything, it too should do as well as possible as often as possible. That is to say, it too should deal with its task statistically."

In the manner described, communication theory may well contribute to the efforts, stressed in the present paper, to determine the structural and functional properties of the unit of behavior in abstract terms. Such determination will in turn contribute toward an explicit recognition not only of the rules and restrictions but also of the licenses and liberties of the objective as well as of the molar approach. It will further contribute to the much-needed establishment of psychology as a discipline of distinctive, well-circumscribed internal coherence and formal unity of purpose within the more broadly unitary framework of science at large.

Bibliographical Notes

Chapter I

1. Of the problems subsumed under "general methodology" and presented in the first three chapters, those of the status of intervening variables, of strict versus probability laws, and of the field and achievement concepts in psychology have been discussed in a Symposium on Psychology and Scientific Method, held at the Sixth International Congress for the Unity of Science, University of Chicago, 1941, with Brunswik, Hull, and Lewin as the participants (*Psychological Review*, Vol. L, No. 3 [1943]); for further discussion of intervening variables and the nomothetic point of view see K. W. Spence, "The Nature of Theory Construction in Contemporary Psychology," *Psychological Review*, Vol. LI (1944), and "The Postulates and Methods of 'Behaviorism,'" *ibid.*, Vol. LV (1948). "On a Distinction between Hypothetical Constructs and Intervening Variables" see K. MacCorquodale and P. E. Meehl (*Psychological Review*, Vol. LV [1948]). (All papers listed in this paragraph are republished in Marx.52)

The interest in "operationism" follows the appearance of P. W. Bridgman's *Logic of Modern Physics* (New York, 1927). For an early survey of the repercussions of this and of related movements upon psychology see S. S. Stevens, "Psychology and the Science of Science," *Psychological Bulletin*, Vol. XXXVI (1939); also of importance is G. Bergmann and K. W. Spence, "Operationism and Theory in Psychology," *Psychological Review*, Vol. XLVIII (1941) (both papers also in Marx52), and a Symposium on Operationism in which psychology is represented by Boring, Pratt, Skinner, and Stevens, and the philosophy of science in the broader sense by Bridgman and Feigl (*Psychological Review*, Vol. LII, No. 5 [1945]) (for a discussion of "levels of explanation" see Feigl's "Rejoinder"). Relations to the logical positivism of the "Vienna circle" are discussed by H. Feigl, "Logical Empiricism," in *Twentieth Century Philosophy*, ed. D. Runes (New York: Philosophical Library, 1943); this and the above-mentioned papers by Feigl are also found in *Readings in Philosophical Analysis*, ed. H. Feigl and W. Sellars (New York: Appleton-Century-Crofts, 1949). See further Bloomfield (this *Encyclopedia*, Vol. I, No. 4, p. 13). For the general aspects of the "quest for certainty" see J. Dewey's classic bearing the same title (London: Allen & Unwin, 1930).

2. See H. Reichenbach, "Rationalism and Empiricism: An Inquiry into the Roots of Philosophical Error," *Philosophical Review*, Vol. LVII (1948). For general orientation see B. Russell, *History of Western Philosophy* (New York: Simon & Schuster, 1945), pp. 564 ff. See also K. J. W. Craik, *The Nature of Explanation* (Cambridge: Cambridge University Press, 1943), p. 10.

3. The standard text in *Experimental Psychology* by R. S. Woodworth (New York: Henry Holt, 1938) contains reference to the majority of experimental findings referred to in this monograph. Thinking, including the completion theory (in the Würzburg version) and the problems of phenomenological introspection, is treated in chapter 30 (quotation from p. 788). The first half of chapter 6 gives a vivid description of the shift from peripheral to the central emphasis in the behavioristic study of maze learning. On learning see further E. R. Hilgard, *Theories of Learning* (New York: Appleton-Century-Crofts, 1948).

4. See A. F. Bentley, "The Human Skin: Philosophy's Last Line of Defense,"

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Philosophy of Science, Vol. VIII (1941). In another paper, published by the same author jointly with John Dewey, under the title "Transactions as Known and Named," *Journal of Philosophy*, Vol. XLIII (1946), esp. pp. 535–36, 541 ff., 546, a "transdermally transactional" point of view is taken which has much in common with the central-distal functionalism described later in the present monograph. For an application to linguistics see the same authors' paper on "Specification," in the same volume. See also their *Knowing and the Known* (Boston: Beacon Press, 1949).

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6. R. Carnap, "Testability and Meaning," *Philosophy of Science*, Vol. III (1936) and Vol. IV (1937), esp. pp. 420, 440, 449, and 466.

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8. E. Frenkel-Brunswik, "Intolerance of Ambiguity as an Emotional and Perceptual Personality Variable," *Journal of Personality*, Vol. XVIII (1949).

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10. M. Schlick, *Allgemeine Erkenntnislehre* (Berlin: Springer, 1918; rev. ed., 1925); A. S. Eddington, *The Nature of the Physical World* (New York: Macmillan Co., 1928), chap. xii.

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17. A. P. Weiss, *A Theoretical Basis of Human Behavior* (Columbus, Ohio: Adams & Co., 1925), pp. 346–47.

18. W. S. Hunter, "The Psychological Study of Behavior," *Psychological Review*, Vol. XXXIX (1932).

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Chapter III

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42. D. B. Klein, "Psychology's Progress and the Armchair Taboo," *Psychological Review*, Vol. XLIX (1942); republished in *Twentieth Century Psychology*, ed. P. L. Harriman (New York: Philosophical Library, 1946).

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62. In a Symposium on Psychoanalysis as Seen by Analyzed Psychologists, inaugurated by G. W. Allport (*Journal of Abnormal and Social Psychology*, Vol. XXXV [1940]), Boring, J. F. Brown, Frenkel-Brunswik (quotations are from pp. 179–83), Landis, and Murray (quotation from p. 165) represent major psychological movements or emphasize the theoretical aspects of the controversy, while psychoanalysis is represented by Alexander and by Sachs.

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64. S. Freud, "Analysis Terminable and Interminable," *International Journal of Psychoanalysis*, Vol. XVIII (1937).

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66. A. Adler, *The Neurotic Constitution* (New York: Moffat, Yard & Co., 1917); see also Murchison.[51] Cf. C. G. Jung, *Psychology of the Unconscious* (New York: Moffat, Yard & Co., 1916).

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67. E. Spranger, *Types of Men* (New German ed., 1921; trans. from 5th ed., 1928). See also H. Kliiver's contribution to the earlier editions of Murphy.[50] For the general history of personality psychology see G. W. Allport, *Personality* (New York: Henry Holt, 1937), chap. iii.

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A history of "Psychological Objectivism" is given by C. M. Diserens, *Psychological Review*, Vol. XXXII (1925). Concerning the behavioristic character of ancient Chinese psychology see S. Fernberger, "Behavioristic Categories as Determiners of Psychological Systems: An Excursion into Ancient Chinese Psychology," *Psychological Review*, Vol. XLII (1935).

71. The major treatises by J. B. Watson are *Psychology from the Standpoint of a Behaviorist* (Philadelphia: Lippincott, 1919), in which a scrupulous attempt is made to avoid terms that imply consciousness and the conception of the human individual as a stimulus-response machine is developed, and *Behaviorism* (New York: Norton, 1924; rev. ed., 1930), which contains not only the most radical exposition of Watson's environmentalism but also the most accurate statement of his theoretical position.

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Chapter V

73. The distinction between "central" and "peripheral," an old standby in general psychology, has been emphasized as a watchword in modern system-making especially by Murray.[84] "Distal" versus "proximal" stimulus reference is the leading motive in a discussion and classification of contemporary schools by F. Heider, "Environmental Determinants in Psychological Theories," *Psychological Review*, Vol. XLVI (1939).

74. E. C. Tolman, *Purposive Behavior in Animals and Men* (New York: Century Co., 1932; republished by University of California Press, 1949); concerning the definition of purposive behavior and the preceding efforts by R. B. Perry ("Docility and Purposiveness," *Psychological Review*, Vol. XXV [1918]), and by McDougall, see pp. 14 ff.

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75. K. S. Lashley, *Brain Mechanisms and Intelligence* (Chicago: University of Chicago Press, 1929).

76. D. Katz, *Die Erscheinungsweisen der Farben* (*Zeitschrift f. Psychol. Supplement*, 1911; Engl. trans. from 2d ed., *The World of Colour* [London: Kegan Paul, 1935]); K. Bühler, *Die Erscheinungsweisen der Farben* (Jena: Fischer, 1922).

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78. E. C. Tolman and E. Brunswik, "The Organism and the Causal Texture of the Environment," *Psychological Review*, Vol. XLII (1935).

79. J. v. Uexküll, *Umwelt und Innenwelt der Tiere* (Berlin: Springer, 1909). See further J. v. Uexküll and F. Bock, "Vorschläge zu einer subjektbezogenen Nomenklatur in der Biologie," *Zeitschrift für die ges. Naturwissenschaft* (1935).

80. B. F. Skinner, *The Behavior of Organisms* (New York: Appleton-Century, 1938). Short-cutting of the organism by the psychologist is first suggested in the same author's "The Concept of the Reflex in the Description of Behavior," *Journal of Genetic Psychology*, Vol. V (1931).

81. A brief, nontechnical introduction to factor analysis is L.L. Thurstone's address, "The Vectors of Mind," *Psychological Review*, Vol. XLI (1934). Further general discussion, including the defense (chiefly against Godfrey Thomson, R. C. Tryon, and A. Anastasi) of the factors as real "functional unities" of a dispositional kind, is found in his "Shifty and Mathematical Components," *Psychological Bulletin*, Vol. XXXV (1938), and "Current Issues in Factor Analysis," *ibid.*, Vol. XXXVII (1940). For a presentation of methods see his *Multiple Factor Analysis* (rev. ed.; Chicago: University of Chicago Press, 1947), quotations from pp. 70, 145, and, for major results, his *Primary Mental Abilities* (Chicago: University of Chicago Press, 1938).

From the point of view of logical positivism, a discussion of problems of individual differences has been given by C. G. Hempel and P. Oppenheim, *Der Typusbegriff im Lichte der neuen Logik* (Leiden, 1936).

82. E. Heidbreder, "Toward a Dynamic Psychology of Cognition," *Psychological Review*, Vol. LII (1945).

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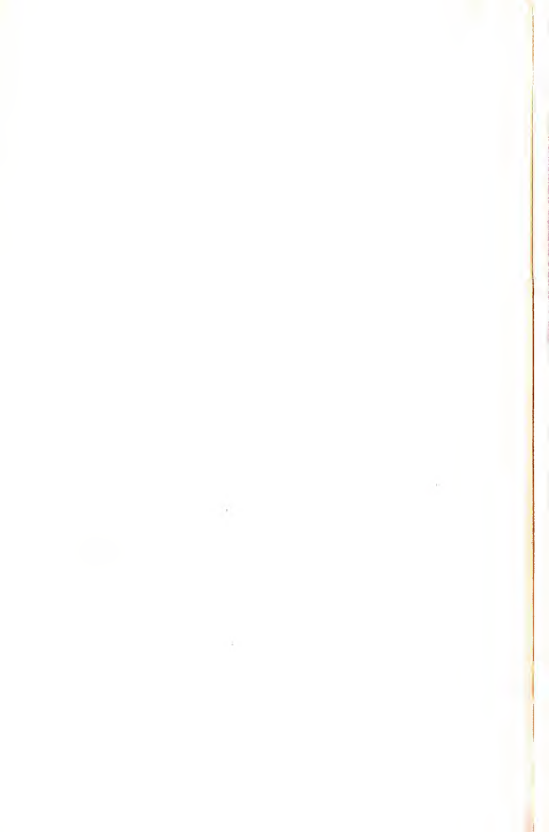
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